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Vol. 192 SEPTEMBER, 1921 No. 3

Journal of The Franklin Institute

1921

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Published Monthly by

THE FRANKLIN INSTITUTE OF THE STATE OF PENNSYLVANIA, PHILADELPHIA

SIX DOLLARS PER YEAR (Foreign Postage Additional) SINGLE NUMBERS, SIXTY CENTS

Entered at the Post Office at Philadelphia, Pa., as second-class matter.

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Acceptance for mailing at special rate of postage provided for in section 1103, Act of October 3, 1917, authorized on July 3 1919.

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Journal
of
The Franklin Institute
Devoted to Science and the Mechanic Arts

Vol. 192 SEPTEMBER, 1921 No. 3

STUDIES IN THE FIELD OF LIGHT RADIATION.*

By CHARLES FABRY, D.Sc.
University of Paris, Honorary Member of the Institute.
Translated by Joseph S. Ames, Ph.D., LL.D.
The Johns Hopkins University, Member of the Institute.

THE direct study of the phenomena of light, independently of any hypothesis, leads to the following conclusion: "Light is produced by a periodic phenomenon which is propagated."

Every radiation, however complex it may be, may be decomposed into a certain number, finite or infinite, of such periodic disturbances; each of these is the simple element of every radiation. I propose to give in what follows a review of the researches, carried on since the days of Newton, which have led to the discovery of all possible types of radiation, and have made this subject one of the most immense fields of study that can be met in physics.

Let us recall the essential elements characterizing every propagation of a periodic phenomenon. They are three in number:

1. The *period* is the essential element of every periodic phenomenon, and is independent of the mode of propagation. The number of periods per second, *i.e.*, the *frequency*, can also serve to characterize this element;

2. The *velocity*, on the contrary, characterizes the propagation, independently of any idea of periodicity;

3. From the periodicity in time and the uniform propagation in space results a periodicity in space, characterized by the wavelength, *i.e.*, the distance the disturbance is propagated during one

* Presented at the Stated Meeting of the Institute held Wednesday, May 18, 1921.

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period. This quantity is nothing but the space-periodicity; two points, at a distance apart equal to any number whatever of wave-lengths, are the seat of concordant phenomena. These three elements, T, V and λ, are evidently connected by the relation $\lambda = V\,T$. If two of them are known, the third may be deduced.

These three quantities are found in the most diverse phenomena, whenever there are at the same time periodicity and propagation. They are found in the propagation of sound, in the periodic motion of a long cord; simpler still, they are found in the displacement of a material body which moves with a periodic change of shape. We may take as the simplest illustration a man who walks taking equal steps. His body, by itself, has a periodic motion; the wave-length is a double step; in two positions separated by a multiple of this length the body is in the same position.

In the cases cited the three quantities, T, V, λ, may be measured directly; no one of them is so small as to escape our means of observation. The condition is not the same in luminous phenomena. In this case the velocity is enormous, and the wave-length, although measurable, is very small. It follows that the period is extraordinarily small, or, what is the same thing, the frequency is immensely great; this time periodicity is completely beyond our powers of direct observation. Let us state the order of magnitude of these various quantities. In a vacuum the velocity is 300,000 kilometres per second; that is to say, in one second light traverses a distance nearly equal to that from the earth to the moon. For a radiation producing in our eyes the sensation of green, the wave-length is in the neighborhood of half a micron (1 micron = $\frac{1}{1000}$ of a millimetre = $\frac{1}{25000}$ of an inch). The frequency, then, equals the number of half microns contained in 300,000 kilometres. If we again consider the comparison with an animal that is walking, it is necessary to suppose that, taking steps half a micron long, it reaches the moon in one second. We can picture that the motion of its legs would be extraordinarily rapid, and that it would be impossible to see the movements; if we were to see this extraordinary creature passing, it would be impossible to say whether it was running or sliding on its path. Only by examining the tracks of the minute steps in the ground could one learn the mode of advance, and only by comparing the length of the steps with the velocity of advance could one calculate the enormous number of steps made each second.

From these details there results a fact which is most interesting. All the equations of optics contain the time as the fundamental variable; yet optics is the only part of physics in which a chronometer is scarcely ever used. Time itself enters into only one experiment of optics, the determination of the velocity of light. This is a measurement which is rarely repeated, and only then when we believe that we have improved our methods of measurement. We determine often, under the name, "indices of refraction," the ratios of the velocities of light in different media, but this does not require an instrument for measuring time. As for the values of the periods, it is never found necessary to calculate them. For every simple radiation, it is the wave-length that experiments give us; and it is this quantity (taken in a definite medium, *e.g.*, air) that is given in order to specify the radiation.

The ensemble of radiations thus forms a continuous series; each is defined by a value for its wave-length; and we do not have any difficulty in picturing all possible values, from the largest (frequency relatively very small) to the smallest (frequency increasing indefinitely). The range actually explored is nearly infinite.

One region, that of radiations which affect our eyes, was explored as a consequence of Newton's discoveries. The first determination of wave-lengths would naturally follow the discovery of the individuality of simple radiations. On studying the colors of thin plates, Newton himself recognized the periodic constitution of the light, and measured, under the name of "intervals of fits," the half wave-lengths of the various radiations. It is interesting to note that the concept of periodicity was introduced into optics by an advisary of the wave-theory, while the ideas are much less clear with the first defenders of this theory; thus the concept of periodicity was not recognized by Huygens, and the ideas of Euler are quite contrary to the facts of experiment.

The numerical results obtained by Newton remained unused, and even misunderstood, during a century. It was not until the work of Young and Fresnel, at the beginning of the nineteenth century, that the concept of periodicity, attached to each simple radiation, assumed all its precision and importance. It was sufficient to explain all the great variety of phenomena known henceforward under the name of interference phenomena. Each of

these gives a value for the wave-length of the light used; having proved that, for red light, all the phenomena led to the same value for its wave-length, identical with that given by Newton, Fresnel simply doubled Newton's intervals of fits so as to have the wave-lengths of the different colors. An investigation made with greater precision would have been, at this epoch, without meaning; neither the possibility nor the need of accurate measurements was recognized. The essential point, definitely acquired, was this: The wave-lengths of luminous radiations form a continuous progression from the violet to the red, from about 0.4μ to about 0.8μ.

A long series of investigations, extending nearly over a century, contributed an ever-increasing power in the means of separating and analyzing the radiations, a realization more and more perfect of monochromatic radiations, a greater and greater precision in the measurement of wave-lengths. I cannot here give the story of this beautiful series of investigations; it will be sufficient to recall the names of Fraunhofer, of Fizeau, of Foucault, of Kirchhoff and Bunsen, of Angström, of Rowland and of Michelson. From the standpoint of precision measurement, it can be stated that the era of progress is closed. Wave-lengths may be compared with each other with the same precision as that in which the wave-lengths are themselves defined; moreover, they may be compared with the unit of the metric system with an accuracy equal to that with which the metre is itself defined, that is to say, with an uncertainty scarcely exceeding one part in 10,000,000.

In expressing wave-lengths one makes use, naturally, of units derived fom the metric system; but it is necessary to choose a submultiple of the metre small enough for the numbers to be convenient. When great precision is not sought, the familiar unit, the micron, is convenient. For exact figures the usage is established of taking a unit of 10,000 times smaller, called an angström (abbreviation A). We have then

$$1 \text{ angström} = 10^{-4} \text{ micron} = 10^{-8} \text{ centimetre} = 10^{-10} \text{ metre}.$$

The wave-lengths of visible radiations are thus found to lie between 4000 and 8000 angströms. It is found that this unit, selected at a time when one knew nothing accurately about molecules, is of the order of magnitude of molecular dimensions, and that it is suited also for studies of atoms and of X-rays. The

atomistic physicists often, in fact, use it, but keep for it the name which connects it with the metric system, namely "tenth metre."

All this development of methods of measurement was made by means of visible radiations, the study of which is the easiest. At the same time progress was made in the discovery and the exploration of other radiations which the eye does not perceive; methods invented for the visible radiations were applied immediately to these new ones.

The existence of non-visible radiations, the natural extension of the series of luminous ones, was revealed as soon as one wished to study the properties of light other than visual. This led to the use of receiving apparatus other than the eye, less specialized than it. Can there exist a universal receiving instrument, that is to say, one which is sensitive to every radiation, whatever its wave-length is? Certainly yes; such a receiver is supplied by a thermometric apparatus, which absorbs radiation; transforms its energy into heat, and measures the quantity of heat liberated. From a certain point of view, every radiation may be considered as bringing about a transfer of energy from the source of radiation to any point whatever of the surrounding space. If this radiation is destroyed by absorption, its energy is not lost (energy is indestructible); it is transformed into heat, and the number of calories liberated per second serves as a measure of the intensity of the radiation absorbed. All radiations are thermal ones, and to an equal degree if of the same intensity. The expression, "heat radiation," is often used; if it is used to designate *any* radiation, it is a pleonasm; if one wishes to designate by it a definite category of radiations (the infra-red, most often) it implies a false idea; these radiations not having any privilege in this respect. It is found, merely, that certain infra-red radiations are often more intense, and therefore give greater thermal effects than others.

These ideas which are evident now were not always so, although the heat effects of radiation have been known for a long time. Without going back to Archimedes, the experiments with burning mirrors often attracted public curiosity in various countries in the eighteenth century. The systematic exploration of the spectrum from the thermal point of view seems to have been made for the first time by William Herschell, in 1800; the prolongation of the solar spectrum beyond the red at once became evident. The exact meaning and the importance of this

experiment were realized only gradually; it was necessary for attention to be drawn as the result of the work of physicists of the highest rank—among them Ampère—to the complete identity of infra-red and visible radiations; it was not easy to accustom people to this idea that we are completely blind for the majority of radiations. However, although the ideas were not always very clear, methods were gradually perfected, by the use of transparent materials other than glass, and by the invention of more and more sensitive thermometric apparatus, the discovery of which is bound up with all progress in the technic of electric measurements. One must come, however, to a comparatively recent period before measurement of wave-lengths in this region of the spectrum can be found; the first are those of Mouton, in 1879, soon followed by those of Langley. Little by little the region explored was prolonged towards the large wave-lengths; the solar spectrum was followed to 5μ, and with artificial sources Rubens was able to measure wave-lengths running to 300μ, almost 400 times as great as the longest visible waves. Beyond this the intensities of the radiations emitted by our sources of light are probably very weak, and we touch probably the real limit of emission spectra; on this side, the *exploration* of spectra is probably complete. Unfortunately it is not as easy as one could wish, and there remain many properties to be discovered, owing to the lack of suitable means of observation. The thermometer is the " standard " instrument for the measurement of intensities; it alone gives a direct measurement of energy; but as a detecting instrument, even in a perfected form, the thermometer remains greatly inferior to the eye or to the photographic plate with reference to usefulness, sensitiveness and fineness of definition; great progress in these respects is still desirable.

On the side of the shorter wave-lengths, beyond the violet, the discovery of the invisible radiations might also have been made by means of the thermometer; it was not thus made because the intensity of these radiations in terms of energy is ordinarily feeble, and more sensitive detectors could be used immediately. The means universally adopted for the study of ultra-violet radiations consisted in using photographic processes, the development of which was in four steps: Daguerre plates (Daguerre, 1829); wet collodion (Talbot, 1839); bromogelatine (gradual development from 1850–1880); orthochromatism (Vogel, 1873). We are

familiar with the revolution accomplished in the study of luminous phenomena by the constant use of photography; in practice it is used for the study of ultra-violet and visible radiations, and even for the first part of the infra-red—up to more than 9000 A—it is the ultra-violet which has benefitted most by the use of photography.

If one passes progressively towards the short wave-lengths, the study of the ultra-violet begins with a minimum of difficulty. The limit between the violet and the ultra-violet radiations is not a frontier either from a theoretical viewpoint or from a technical one; there is no difference in the nature of the two types of rays; and the methods used for their study are exactly the same. In proportion to the distance one advances towards the shorter wave-lengths the difficulties increase. About the wave-length 3400 A, glass begins to become strongly absorbing and it is soon necessary to give up using it; happily certain crystallized substances, of which quartz is most often used, preserve their transparency and replace glass. About 2900 A the solar spectrum ends, owing to absorption by the ozone in the higher levels of the earth's atmosphere; numerous artificial sources keep a great intensity. Without too much difficulty, one can photograph and measure radiations whose wave-lengths are as small as 1850 A; this is the limit reached by Cornu working from the year 1881 on. Starting from there, the difficulties suddenly become so great that one can ask if it is possible to overcome them. Quartz, in its turn, becomes strongly absorbing, and it is necessary to give up the use of this fine substance; the gelatine of the plates becomes so opaque that the radiation does not penetrate it and no longer acts upon the silver salt; finally, the most serious difficulty of all, the air itself ceases to be transparent and absorbs all the radiations by a very small thickness. Cornu, after making clear the causes of the limitation of the spectrum in his experiments, seems to have given up the attempt to overcome the difficulties which arose.

They were overcome a little later by Schumann in 1890. The three parts of the problem were solved in the following manner: (1) Quartz was replaced by fluorite, whose transparency extends much farther; (2) photographic plates without gelatine were obtained; (3) the apparatus was placed in a vacuum. The spectrum of hydrogen obtained under these conditions showed an enormous extension compared to what had been known; it was limited

solely by the opacity of fluorite, which in its turn ceases to be transparent at $\lambda = 1200$. Moreover in the experiment by Schumann, the wave-lengths were not measured, only estimated, using an extension of the dispersion formula for fluorite.

In order to proceed still further toward the short wave-lengths, in the absence of any known transparent substance, it was necessary to suppress every solid body in the path of the rays; this led to the necessity of rejecting all the ordinary apparatus of optics, both lenses and prisms. There are only two means at our disposal: Reflection by metallic mirrors, and the use of diffraction. This led to the use of the concave grating which had been introduced into optics by Rowland; the use of this apparatus has, at the same time, the advantage of giving directly the lengths of the waves, which the prism does not. With a concave grating in a vacuum, Lyman succeeded in determining the lengths of the waves in the spectra studied by Schumann; then in prolonging these spectra beyond the limit of transparency of fluorite, and finally he reached wave-lengths of 510 A in the spectrum of helium.

A last step in advance has been made recently in the same path by Millikan, using powerful means and a new method to produce the emission. The spectroscopic apparatus is still a concave grating, placed in a high vacuum (pressure 10^{-4} mm. of mercury); the source of light is a high potential spark between metallic electrodes in the same vacuum. The spectra obtained are extremely rich in rays, and extend to 200 angströms.

This is the limit reached to-day in the study of radiations by optical means. It is seen that the study of the ultra-violet has led to an enormous increase in our domain, since on passing from the last visible radiation ($\lambda = 4000$) to the limit reached in the ultra-violet, the frequency increases in the ratio of 1 to 20. Why is the actual limit at $\lambda = 200$? Is it, as at the other end of the spectrum ($\lambda = 3{,}000{,}000$ A), owing to the lack of production of radiation beyond this limit? Is it, in other words, a "production crisis" that stops us? Certainly not; the radiations near the limit are still emitted with great intensity; we are stopped by an accumulation of technical difficulties, and in particular by a "transportation crisis," since the radiations are absorbed by the least trace of matter.

While these beautiful researches were extending the domain of radiations towards the short wave-lengths, a brilliant series of

discoveries in another field revealed to spectroscopists another "promised land." In 1895 the discovery by Röntgen of X-rays had shown that there existed unsuspected radiations. The properties of these seemed to be completely different from those of of optical radiations: Absence of reflexion; propagation in a straight line without refraction or appreciable diffraction; the property of traversing great thicknesses of nearly all bodies, even those which are opaque to all other radiations. The nature of these radiations appeared at first to be absolutely mysterious; it was all the more difficult to prove that there was, in a way, no means of acting on them, and that the ordinary technic of optics was not applicable to them. The absorbing power of various substances was almost the only property that could be expressed by numbers. These feeble means of study were sufficient to prove that X-rays, like luminous ones, were non-homogeneous; and the concept was introduced, almost unconsciously, that they formed also a continuous series of simple radiations each characterized by some peculiarity. The rather vague distinction of soft rays (easily absorbed) and of hard rays (absorbed with difficulty) was introduced into practice; at the same time important studies were made in which each X radiation was characterized by the coefficient of its absorption by a definite substance. As to the nature of these rays, many physicists thought, without being able to give a proof, that the X-rays were light waves having extremely short wave-lengths, that is to say, were a hyper ultra-violet. This hypothesis explained the nearly complete absence of diffraction phenomena, and also the absence of reflection and refraction if the wave-length was of the order of magnitude of molecular dimensions. There seemed to be no hope of measuring the lengths of the waves, on account of the impossibility of ruling sufficiently fine gratings.

Suddenly this chimerical hope was realized by the brilliant discovery of Laue (1912). His bold idea was to take as a grating the regularity offered by nature in a crystal. The early work of crystallographers (Romé de l'Isle, Haüy) had accustomed physicists to consider crystals as having a regular arrangement of particles; about 1850, Bravais had developed completely the mathematical theory of lattice systems in order to explain the shape and the properties of crystals. Laue, familiar with the theory of multiple interference, remarked that such a system should act,

with a radiation having a wave-length comparable with the distance between the particles, like a three dimensional grating. Sending a bundle of X-rays through a crystal, the phenomena expected were immediately realized. A short time later Bragg attacked the problem from a simpler point of view, by studying the phenomena of *reflection* by a crystal, which behaves like a medium made up of layers, analogous to the color photographs of Lippmann, but with the layers pressed together many thousand times.

The enigma of X-rays was from then on cleared up; they form the distant extension of the ultra-violet rays; and the field was thus opened for the spectroscopy of these radiations. It being possible to determine the interval between the lattice planes of a crystal, all the elements were at hand required for the determination of the wave-lengths of the X-rays.

The measurements thus made have given wave-lengths comprised between 10 and 0.05 angström. They are waves of the same order as the dimensions of molecules. The γ-rays emitted by radio-active substances form, beyond any doubt, the natural prolongation of the group of X-rays toward the shorter wave-lengths; the lattice grating of a crystal becomes, in its turn, too large spaced to analyze them. Certain physicists think that the wave-lengths of the γ-rays may be as small as a thousandth of an angström, but this is still hypothetical.

Thus, on descending towards the decreasing wave-lengths, we lose the ultra-violet rays at about 200 angströms, and find the X-rays at about 10 angströms; between the two there is an unexplored region fairly wide. What can exist in this unknown land? Starting from the side of the X-rays and going towards the increasing wave-lengths, one should find radiations more and more easily absorbed; this absorption should then pass through a maximum and gradually decrease. At a certain point (we do not know where), the reflective power of metals should begin to be evident. Refraction, zero for the X-rays and very marked for the ultra-violet rays, should begin to appear in the unknown region; but it will be difficult to note its beginning on account of absorption. All this region will moreover be the most difficult to explore of the entire spectral domain.

Through this difficult "no man's land" a path has just been traced by M. Holweck, working in the laboratory of Mme. Curie. The direct measurement of the wave-lengths has not yet been

made, and will, without doubt, be extremely difficult; but the existence of the radiations intermediate between X-rays and ultra-violet ones has been proved, and some of the properties predicted have been verified. The mode of production adopted is the same as that used for X-rays: The apparatus is a sort of Coolidge tube, made entirely of metal, in which the anode and cathode are separated by the distance of one millimetre. The tube is worked under an exceedingly low potential (between 25 and 1800 volts); the wave-lengths are calculated by the theoretical formula, verified in the case of ordinary X-rays,

$$\lambda = \frac{12{,}400}{E}$$

in which λ is the wave-length expressed in angströms and E is the potential in volts.

As a detector, M. Holweck makes use of the ionizing properties of the rays, which they share with X-rays and ultra-violet ones. Emerging from the tube where they are produced, the rays enter an ionization chamber containing air at a pressure of 1 mm.; the existence of the radiation is manifested by the electric conductivity of this air, as indicated by an electrometer. It was most difficult to separate the ionization chamber from the tube by a wall which is air-tight and yet transparent for the most absorbable waves; the problem was solved by using a window made of celluloid having a thickness of only ¼ micron ($\frac{1}{100{,}000}$ of an inch). It was possible to detect a radiation for E = 25 volts, which corresponds to a wave-length 493 angströms; thus joining the known region of ultra-violet radiations. The absorbing power for various radiations has been measured for several gases and for celluloid; it is found that for this last substance the absorption passes through a maximum for $\lambda = 320$ A; the sheet of thickness one-quarter of a micron permits to pass only three per cent. of this radiation.

It can then be said that from the extremity of the X-rays ($\lambda = 0.05$ A) to the extreme infra-red rays ($\lambda = 3{,}000{,}000$ A) there is but a single field of radiations; from one end to the other, the wave-lengths vary in the ratio of 1 to 60,000,000. On the side of the X-rays, this vast domain is prolonged by the γ-rays, whose wave-lengths have not yet been measured; on the side of the long wave-lengths it nearly makes connection with the electric waves which differ from optical radiations only with reference to their mode of production. The exploration of the domain of radia-

tions is then very far advanced, and one can with a single glance of the eye, contemplate its entire extent and all its beauty.

Can one say that all discovery in this chapter of physics is now ended? Nothing is farther from my thoughts. If the *exploration* of the land is nearly finished, the realization of its importance has only begun. Knowing the methods suited to produce the various radiations, the methods which enable one to analyze and measure them, a vast field of investigation is open to the physicist: The study of the reciprocal relations between the radiations and matter, which offers the most powerful means for penetrating into the knowledge of matter. Now this study has hardly begun. For no substance can one actually indicate, even approximately, the position of *all* its absorption bands; for no body can one trace completely its absorption and dispersion curves. For no substance can one describe with certainty the entire group of radiations that it can emit, from the X-rays up to the extreme infra-red. The knowledge of fluorescence, a most general phenomenon, is still rudimentary.

If this work is not more advanced, it is first of all because being so vast, it can not be finished except after a very long time; but there is another reason. Through a great part of its field, spectroscopy still is a very difficult science; its methods are those of the explorer who is never sure that he will reach his goal, and not those of the ordinary worker who is sure of his means of production. In the region from $\lambda = 2000$ A to $\lambda = 9000$ A, comprising the visible spectrum, a small portion of the infra-red and the easy part of the ultra-violet, we have methods which are certain and relatively convenient; but beyond this narrow interval, the means for working are truly too laborious and too difficult. Many technical improvements are desirable.

The materials used are not always those which one could wish. With the exception of glass, one is almost entirely bound to natural crystals, that is to say, to chance; the number of deposits is very limited, and physicists are at the mercy of an industrial exploitation very limited and very intermittent. Quartz is found without too much difficulty; but fine crystals of calcite, used in many researches, are found only in Iceland, and now this substance has nearly completely disappeared from the market. Fluorite, which for certain purposes cannot be replaced, is a substance very rare and very expensive, and the most beautiful specimens of it

are very small; the number of good prisms of fluorite existing in the world may be counted probably only as a few dozens. In the infra-red where all substances have absorption bands, one is forced to make use of a certain variety of substances; the physicist does not aways find, among natural specimens, those which he wishes. It would be of great interest if one were to find new artificial materials for use in spectroscopy, either among smelted bodies or among crystals. Some attempts made in America to produce fine artificial crystals have given encouraging results; it is to be hoped that these attempts will be pursued methodically with a large number of substances.

Gratings form another important category of instruments for the analysis of radiations. The results obtained in the construction of these instruments are marvellous; the total number of lines ruled on a single plate, spaced at exactly equal intervals, exceeds 100,000. Unfortunately the commercial state of this industry is not good. The production of gratings is exclusively an American industry; no European instrument maker has ever produced a single grating of any value as an instrument of research. At present the demand for gratings far exceeds the supply; and I could name several European physicists whose researches are stopped because they cannot obtain the gratings essential for their work. If the output of gratings could be increased, it would be of great general usefulness for the progress of physics.

Finally there is a great need of perfecting the apparatus for detecting radiation. The eye, and especially the photographic plate, are the best suited; but their range of sensitiveness is limited. The universal apparatus, the thermometer, in spite of the admirable improvements which it has received, is still difficult to use; its sensitiveness is still insufficient, and its power of definition is less than that of the photographic plate. Other detectors have been successfully tried in recent years: Photo-electric cell, selenium apparatus, method based upon ionization. These detectors can render great service for measurements of intensity, but they are also lacking in power of definition; and moreover, their sensitiveness is strongly selective. A very desirable improvement would be the prolongation in the infra-red of the sensitiveness of the photographic plate; very beautiful results have been obtained already by improving their orthochromatism, which has enabled us to push gradually the limit of sensitiveness of the plates from

about 4800 A to beyond 9000 A; and photography at the beginning of the infra-red is now an easy matter. It is an interesting fact that the development of photography, since Daguerre, rests almost entirely upon the properties of silver salts, discovered by accident. Has it so happened that the first accident gives the best solution? One would be tempted to believe so, after the failure of many other attempts. Unfortunately the theory of photographic actions is so little advanced that systematic researches are very difficult. A happy accident may one day or another open some entirely new way.

We see that, if the rôle of the explorer is reaching its end in the domain of luminous radiations, there remains a considerable task for the technician to accomplish.

Quantitative Precipitation of Gold by Means of the Electric Current. W. D. TREADWELL (*Helvetica Chimica Acta*, 1921, iv, 364–374) states that metallic gold may be deposited quantitatively from its solution as a chloride, by means of the electric current, provided the solution contains ammonium acetate plus either hydrochloric or sulphuric acid. Using this electrolyte and the proper voltage, gold may be separated from copper, palladium, and platinum.

J. S. H.

Use of Catalysts in the Alkylation of Aromatic Amines with Aliphatic Alcohols. ARTHUR J. HILL and JOHN J. DONLEAVY, of Yale University (*Jour. Ind. and Eng. Chem.*, 1921, xiii, 504–509), find that the formation of diethylaniline by the reaction between aniline hydrochloride and ethyl alcohol is promoted to a marked degree by certain catalysts such as a combination of sodium bromide, calcium chloride and cupric chloride, or a combination of sodium bromide and a powdered metallic copper. The combination first mentioned also acts as a catalyst in the formation of the tertiary bases from the three isomeric toluidines (methyl, amino benzenes) by their reaction with either ethyl or normal butyl alcohol. The primary base and the alcohol are mixed so that ten molecules of alcohol are present for each molecule of base; the mixture is heated with the catalyst in an autoclave. Orthotoluidine is the least active of the four primary bases—aniline, orthotoluidine, metatoluidine, and paratoluidine. Normal butyl alcohol is less active than ethyl alcohol; however, the actual *increase* in yield of tertiary base is greater with normal butyl alcohol than with ethyl alcohol.

J. S. H.

ELECTRIC TRACTION—A REVIEW.*

By FRANK J. SPRAGUE.

Member of the Institute.

A RECENT announcement states that in the United States alone electric railways cover a trackage of 44,000 miles and represent a capital investment of approximately $5,000,000,000, while 300,000 men are employed and over 14,000,000,000 passengers are carried annually—ten times as many as are carried by the steam railroads of the country.

In addition, there is the like class of roads in foreign countries, and thousands of miles of trunk lines here and abroad which are electrically operated or for which electrical equipment is planned. This is a brief epitomé of one of the most astounding of industrial growths, the product of but a third of a century of effort.

In accepting the invitation to present a paper reviewing this great development I cannot but feel something of that embarrassment which must unavoidably attend the presentation of facts which, if stated correctly from a historical standpoint, must include something of one's personal contributions, especially as one must lack that true perspective for which time, distance and the impersonality of an historian are essential. But, to quote a distinguished predecessor, reminiscence is one of the privileges of advancing years and an accumulation of experience, and is sometimes permitted by a kindly audience.

The history of the electric railway may be considered as covering various periods—such as, the inception of basic principles, the early crude experiments, the invention and evolution of the dynamo-electric machine, the discovery of the principle of self-excitation and the reversibility of function, the scattered attempts at commercial operation, the advent of the modern trolley, the succeeding technical and commercial developments, trunk-line operation and future possibilities. As to these I shall draw quite freely upon my previous papers on the same general subject.

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* Presented at the Stated Meeting of the Institute held Wednesday, May 18, 1921.

DISCOVERIES OF FARADAY AND HENRY.

Whatever moderns may have done, two names stand out in the early dawn of the electric art, undimmed by the glory of any subsequent achievements. On Christmas morn, just a hundred years ago, Michael Faraday, "late bookkeeper's apprentice, and now turned philosopher," announced the all-important discovery that it was possible to produce mechanical motion through the agency of the electric current and magnetic attraction, and ten years later Joseph Henry, then a Professor of Natural Philosophy in the Albany Academy, who had antedated Faraday in the discovery of electro-magnetic induction between unmoved conductors, announced in a letter to Professor Silliman, of New Haven, another advance, as follows: "I have lately succeeded in producing motion by a little machine which I believe has never before been applied in mechanics, by magnetic attraction and repulsion."

But Henry, coolly analyzing the limitations of the electro-magnetic motor, which, however it might be improved, was still dependent upon the burning of zinc in a primary battery, and clearly recognizing the economic fallacy of any attempt to substitute it for coal as a source of energy, avoided committing himself to impracticable attempts for its introduction into the mechanical world.

THOMAS DAVENPORT, THE PIONEER.

It, therefore, fell to one Thomas Davenport, a blacksmith of Brandon, Vermont, of whom it has been said that his was a mind which could not be confined to any shop less than the canopy of Heaven, to throw himself heart and soul into the attempt to create an industrial revolution by electricity—a project that, at the time, was certain to prove illusory; and to this same man, in all justice, must be given the credit, in this country at least, for the first crude suggestion of an electric railway—the operation of a moving vehicle by electric power supplied from a stationary source.

From about 1833, when Davenport began his electrical career, until his early death in 1851, his life was the typical one of many inventors who have blazed the path for others, the active strivings of a fertile mind urged to constant endeavors and dreaming great dreams, finally beaten to the wall, partly because of the lack of financial assistance but more because he lacked the essential of commercial success, a suitable economic source of electric power.

By 1834 he had built his first crude motor; in the next year he devised the commutator; and in 1836–37 he illustrated on a miniature circular track, two feet in diameter, the principle of an electric railway using current from a central source, with return through the rails.

On February 25, 1837, Davenport was granted the first and broadest patent in America on this general subject, the character of which is indicated by the following claim:

> The discovery here claimed and desired to be secured by Letters Patent consists in applying magnetic and electro-magnetic power as a principle for machinery in the manner above described, or in any other substantially the same in principle.

This wording may fairly be declared to cover the whole field of the utilization of electric power.

I have only touched upon Davenport's activities, the number of which are amazing, but refer to his work to emphasize not only its creative originality and importance, but the undoubted fact that had he had the advantages of the developments which followed in the succeeding years in the evolution of the dynamo, the machine for converting mechanical energy into electricity, and the converse, many of the advances now credited to other inventors would undoubtedly have been his.

OTHER EARLY WORKERS.

A close rival of Davenport was the Russian engineer, Jacobi, who also in 1834 had produced an electric motor, and three years later actually propelled a boat on the river Neva with energy from primary batteries. This decade seems to have been prolific not alone in attempts to apply electric motors to industrial purposes but in a belief in its ultimate universality of adoption. One of the most notable attempts made was that of a Scotch engineer, Robert Davidson, who in 1838 began the construction of an electric locomotive which had a test on the Edinboro-Glasgow Railway; and the thought of many workers was well voiced in the remarkable prediction by Lieutenant LeCount of the British Navy, who in a "Treatise on Railways" published by Black, of Edinburgh, in 1839, said:

> We have no hesitation in saying that electro-magnetism will at no distant day compete with steam as a motive power, and successfully.

The use of rails for carrying the electric current is said to have

been indicated in an English patent issued to Henry Pincus in 1840, and a like use in an American patent issued to Lilley and Colton in 1847. Whatever the fact, it is well established that in the latter year, Prof. Moses G. Farmer, who was one of the ablest of early American scientists, operated an experimental car, carrying two passengers, at Dover, New Hampshire, which was supplied by battery current: Three years later Professor Page, of the Smithsonian Institution, with Congressional aid, constructed a number of motors. With one of these, on April 29, 1851, and with current supplied from a battery of one hundred Grove cells carried on a small car, he operated the latter on a road between Washington and Bladenburg, where it got up to a speed of nineteen miles an hour, with resultant destruction of the batteries and an end of the experiment.

In this same year an instrument maker of Boston, Thomas Hall, made a small motor which was operated by current conveyed to the rails from a primary battery. Nine years later he exhibited a model called the "Volta" at the Mechanics Fair in Boston. Some years before this patents issued to inventors abroad had indicated the possibility of taking current from a suspended conductor.

EVOLUTION OF THE DYNAMO.

These various experiments were, of course, doomed to commercial failure, not alone because the motors were of a crude construction, based upon the attraction of keepers or solenoids, but because the source of power was in all cases a primary battery. They were, however, indicative of what would come later when the evolution of the modern dynamo should take place.

Beginning with improvements by Wheatstone and Cook in 1845, and by Hjorth in 1854, there appeared in 1853 an inventor whose name is, unhappily, now not even known, who is reported to have clearly set forth the vital idea of self-excitation of the field magnets.

The modern dynamo-electric machine dates from the epoch-making invention of the continuous current dynamo by Pacinotti in 1861, which was followed by the announcement of the principle of self-excitation, independently and practically simultaneously, by the Englishmen, Wheatstone, Varley and Ladd, the German, Siemens, and the American, Farmer, in 1866–67.

Three years later a Frenchman, Gramme, combined the two vital features in a single machine, and to him is fairly due the credit of producing the first practical commercial machine for continuous current operation, whether used as a dynamo or motor.

The ring form of armature, invented by Pacinotti and improved by Gramme, ultimately gave way to the drum form of winding proposed by Von Hefner Alteneck of the Siemens firm of Berlin, and also independently by the late Prof. Henry Rowland of the Johns Hopkins University.

REVERSIBILITY OF FUNCTION.

Some time, apparently, relapsed after the development of the self-exciting machine before the remarkable characteristic of the reversibility of function was discovered, with the necessary corollary, the electric transmission of energy by the use of two similar machines connected in the same circuit, one driven by power and generating electricity, and the other receiving electricity and developing mechanical power.

It is claimed that this vitally important fact was discovered by Pacinotti in 1867, but it is certain that for the time being no use of it was made. In 1873, however, Messrs. Gramme and Fontaine demonstrated it at the Vienna Exhibition, where a number of Gramme machines were in use. This discovery is said to have been due to the mistake of a workman who coupled a machine to a live circuit and was astonished to see it begin to rotate. This is quite likely, as the same thing has happened in recent years. Gramme instantly saw the significance of this unexpected occurrence, concerning which the great Maxwell expressed the opinion that it was the most important discovery of modern times.

In the nearly quarter of a century ending in 1875, during which period the modern dynamo and motor had been created, there appeared to be a complete cessation of electric railway experiments. It was not until the latter year that George Greene, a mechanic of Kalamazoo, Michigan, is reported to have built a small motor which was supplied from a battery through an overhead line, with track return, and three years later constructed another larger model. Lacking appreciation of the necessity of using a dynamo for a generator of electricity instead of a battery, or possibly being

without means to get one, his work came to naught. Greene applied for a patent in 1879, and was finally allowed some very broad claims.

FIRST SIEMENS' WORK.

Among the European concerns engaged in buiding dynamos for electric lighting and other purposes, that of Siemens was the most prominent, and naturally the transmission of power soon commanded its attention, with the result that in 1879 this firm showed at the Berlin Exhibition a small car, operating on a track about a third of a mile long, which used a dynamo-electric motor and took current from a third rail, with track return, supplied by a stationary dynamo. On this locomotive the motor was carried longitudinally, the revolutions of the armature being transmitted through spur and beveled gears to a central shaft, from which connection was made to the wheels. Three small cars carrying about twenty people were pulled by it at a speed of eight miles an hour.

In the next year Egger exhibited a model in which the current was supplied through the running rails, and Messrs. Bontemps and Desprez, of Paris, proposed a scheme for the use of miniature electric locomotives in place of the pneumatic transmission of dispatches. This is the same Desprez who actively conducted some of the earliest important experiments in the transmission of power over long distances with direct current at high potentials.

ADVENT OF FIELD AND EDISON.

While these developments were going on abroad two American inventors, the late Stephen D. Field and Thomas A. Edison, began electric railway experiments, but, subject to the claims made for Greene, the credit of the first serious attempt in the United States at this period must be awarded to Field, who, while living in San Francisco in 1877, is reported to have conceived the idea that electric power might be substituted for the cable then used to propel street cars in that city. To carry out some proposed experiments, the first of which was to operate an elevator, he ordered a dynamo machine from Europe. This being lost he ordered a second one. In 1879, his resources being exhausted, he returned to New York and laid his plans for the electric operation of cars with current supplied through a conductor carried in

a slotted conduit before the late Franklin Leonard Pope. In this year he filed a caveat, and followed it by a regular patent application in the next year. This disclosed plans for an electric railway designed to use current from a stationary dynamo, transmitted through a third rail or an insulated conductor enclosed in and protected by a conduit, the traffic rails, divided into sections, forming the return circuit.

Mr. Edison, at this time, was within reach of great electric possibilities, although actually missing them, for in the face of much adverse criticism he had developed, in connection with his electric-lighting system, a low, internal, resistance dynamo with high-resistance shunt field and many of the features of the multiple-arc system of distributon, analogous to that used in gas and water supply.

In 1880 he built a small road at his laboratory at Menlo Park, on which he ran a single locomotive equipped with a lighting dynamo used as a motor. The power was transmitted to the car axle by a belt and the two rails were used as conductors, one set of wheels being insulated. In this year Siemens, Field and Edison became involved in a patent interference. Siemens being ruled out by office limitations as to evidence relating to work abroad, the immediate issue narrowed down to Field and Edison and was decided in favor of the former, to whom was issued a patent covering " the combination of an electric motor operated by means of current from a stationary source of electricity conducted through the rails," a claim manifestly absurd in view of the previous art and void on the ground that the mere substitution of a dynamo in the place of a battery did not constitute invention.

In general, however, because of the facts of equivalency, and also on account of the records of early experimental work, the claims obtained in the patents of this period were distinguished by their paucity and limitations.

FIRST COMMERCIAL ELECTRIC RAILWAY.

The demonstration by Messrs. Siemens and Halske in Berlin in 1879 was followed by other exhibitions by them at Brussels, Düsseldorf and Frankfort, and then they established a short one-car line a mile and a half long at Lichterfelde, near Berlin. This small road was opened for traffic in May, 1881, and may be considered the first commercial electric road on which fares were

collected. The motor was carried on a frame beneath the car body between the axles, the power being transmitted by steel cables from the armature to drums on the axles. The car had a capacity of twenty-six passengers and could make a maximum speed of about thirty miles per hour. The electrical pressure was low, only about 100 volts, and the two rails were used as conductors. This line continued in actual service for a number of years, the method of current supply, however, being later replaced by two conductors carried on tops of poles, upon which ran a small carriage connected with the car by the flexible cable.

This equipment was followed by one at the Paris Exhibition, where overhead distribution was used for the first time; the conductors in this case consisting of two tubes slotted on the under side and supported by wooden insulators, which tubes contained skids held in contact by an under-running wheel pressed up by springs carried on a frame-work supported by the conductors. Connection with the car was made by flexible conductors, and the power from the motor, which was carried between the axles, was transmitted by a flexible chain.

About this time Field constructed and put into operation an electric locomotive on a track in Massachusetts, but this experiment was soon abandoned.

SERIES-PARALLEL CONTROL.

Up to 1881 all proposals to use motors were based upon their individual and separate control, but Dr. John Hopkinson, an English scientist of exceptional mathematical and scientific knowledge, in a patent for the application of motors to hoists, proposed both for them and for tramways the grouping of two motors in series-parallel arrangement, by which method either two motors, or one motor with two armature circuits, could be run at half or full speed by providing a switch so that the current from a source of constant potential could be sent through the two circuits in series or parallel.

In the same year, and quite independently, I contructed a small dynamo at the Torpedo Station, Newport, which had two armature circuits and a switch by means of which like series-parallel combinations could be made, and tests of the machine under these conditions were conducted, first as a dynamo and later as a motor. The direct change-over from one combination

to the other, however, with high electrical pressures or large currents, would be too abrupt and destructive of the controlling switch, and so this practice did not come into commercial use until some time later, when there was combined with the simple series-parallel control a variation of current strength by a resistance in the circuit of the motor, either by varying the arrangement of section of field coils or the amount of external resistance, thus making the change-over easier and giving additional speed increments.

The two functions being progressively effected by Condict by the movement of a single handle, his controller as modified by the addition of the magnetic blowout by Thompson and Potter became a practical feature in everyday operation, and for a long time the validity of patent covering this combustion was upheld. A final adjudication, on the discovery of an Austrian patent issued to Reckenzaun, who had conducted a number of experiments with storage-battery cars, was probably avoided by a combination of the contending interests.

FURTHER ACTIVITIES.

The year 1882 was marked by an increase in electric railway activities. In Europe, the Siemens firm constructed an experimental road near Meran in the Tyrol to demonstrate the possibilities of electric traction for the St. Gotthard Tunnel, and small lines at Frankfort and Molding, followed by a proposal for a combined elevated and underground railroad for Vienna.

In the fall of that year Doctor Finney, of Pittsburgh, devised a system for operating electrically propelled omnibuses with current taken from overhead wires carrying a small trolley connected to the vehicle with a flexible cable. About the same time in England, Professors Ayrton and Perry read a paper on Automatic Railways before the Royal Institution, and Dr. Fleeming Jenkin, the distinguished Scotch scientist, proposed a telepherage system, or automatic overhead railway for carrying merchandise, plans for which were developed by them in concert.

In the same year, Mr. Edison, at the instance of the late Mr. Henry Villard, undertook further experiments at Menlo Park, but save in extent there was little of novelty shown. Both Field and Edison took out some additional patents, one of the latter's being for the control of motors on different cars by a single main controller on a pilot locomotive, a plan never put in

use. Although Field later undertook some isolated experiments, and Edison, at a much later date, essayed an unfortunate attempt to use rail supply at very low potential differences as a substitute for the trolley, both inventors soon ceased to become active factors in the development of the art.

THE MODERN TROLLEY.

Opportunity is often responsible for inventions, and it was largely so for my entry into the electric railway field. As a cadet at the United States Naval Academy in 1874–78 I had become intensely interested in various electrical inventions, and in 1879 and 1881 I had actively begun electrical experiments, the intervening year being spent on a cruise around the world. While on duty on the training ship *Minnesota* in the latter year I made an attempt to introduce the incandescent electric light into the Naval service, current to be supplied from an Edison dynamo driven by a single-cylinder, fly-wheel pump which I intended to divert from its normal duty. But not effecting the loan of a machine this ambition was for the time not realized.

The principal activities of this period concerned the electric lighting field, and were marked by sharp recriminations between the advocates of series arc lights operated wth a constant current at a high pressure and the multiple-arc distribution of high-resistance incandescent lamps at a constant pressure. The application of electricity to motive power seemed to have been largely lost sight of, and the various companies which were formed manifested their limitations by their names, being generally known as "illuminating" or "lighting" companies.

My ambitions being aroused by the opening of the Paris Electrical Exhibition in 1880–81, I terminated my experiments at the Torpedo Station and applied for orders to Paris, failing which I procured an assignment to the *Lancaster*, sailing for the Mediterranean, with leave on arrival, and subsequent orders to the Electrical Exhibition held in 1882 at Syndenham, England. As secretary of a jury I had charge of the testing of dynamos and gas engines. To the best of my recollection there were no stationary electric motors exhibited, but there were many lighting exhibits, the principal one being Edison's, and of course there were no electric railways. While riding upon the underground road I conceived an idea of electric propulsion based upon the use of the

tracks as one conductor, and for the other a system of rigid overhead rails, all in one plane, following the centre line of all tracks and switches, contact with the same to be made by an upward-pressing wheel or cylinder carried by the car over the centre of the truck and supported by springs with lateral movement so as to accommodate the motion of the car upon its springs. This plan followed an earlier suggestion to use a conductor carried between the rails, discarded because of the great complexity of switches which I noted at one of the principal stations.

Resigning from the Naval service, with a year's leave, I returned to the United States in the spring of 1883 to become an assistant of Mr. Edison in connection with his electric light station, and while engaged in this duty I developed a mathematical means for determining and improving the system of central station distribution. I found time, also, to conduct experiments with electric motors, and as a result of my awakened ambition in this field I had a clash with my employer within a year, and formed the Sprague Electric Railway and Motor Company, which was destined to play a leading part in the advancement of the art.

Meanwhile the Edison and Field interests had combined in the Electric Railway Company of the United States, its first move being the exhibition of a small locomotive called the "Judge," which run around the gallery of a building at the Chicago Railway Exhibition. A Weston dynamo was used for the motor, geared to a jackshaft from which power was transmitted to the wheels by belts. The current was supplied from a centre rail, with track return, a lever operated clutches on the driving shaft, and the speed was varied by resistances in the circuit. For reversing the motor two sets of brushes were used, only one pair of which could be thrown in circuit at a time by a lever and disc geared to the brush-holding arms.

VAN DEPOELE'S ACTIVITIES.

Early in this year a Belgian woodworker, Charles J. Van Depoele, who was interested in electrical work, attacked the railway problem in energetic fashion, and being a tireless worker and prolific inventor, left a permament impress upon it. His first experiments were conducted in or near his works in Chicago, in the winter of 1882–83, where a car was operated by an arc-light dynamo used as a motor, the current being taken from a wire laid

in a trough; and in the autumn the car was shown at the Chicago Industrial Exhibition. This venture was followed by considerable work to which reference will be made later.

About this time Van Depoele is reported to have tried an experiment in which he used an overhead wire with an under-running contact wheel, and this formed the basis of an interference between us, involving the issue of the under-running trolley. Neither of us had applied for patents until about 1886, and while each had made the invention quite independently, it is a moot question if my conception while abroad was not the earlier and more complete. In the interference proceedings, however, despite the fact that I was a Naval officer and had been abroad on duty, the Patent Office ruled that my testimony as to the conception of invention could only be accepted as of the date of my return to the United States in May, 1883. On the basis of this ruling concession of issue of the patent was yielded to Van Depoele, but on account of the closeness of the dates of conception, and possibly because of doubt as to the legality of the Patent Office action, this was on condition that the Sprague company should have free license to use the under-running trolley, while certain special features covering the working conductors and feeders were not reciprocally permitted.

LEO DAFT'S EXPERIMENTS.

Among the active American workers of this period was Leo Daft, who after engaging in the development of motors for stationary work had applied himself to the electric railway problem, making his first experiments at his company's works in Greenville, New Jersey, in 1883. These were resumed in November of that year on the Saratoga and Mt. McGregor Railway, where a locomotive called the "Ampère" pulled a full-sized car. The motor was mounted on a truck and connected by belts to a jack-shaft, from which other belts led to pulleys on the driving axles. The current was supplied from a central rail, with the running rails for return, and at comparatively low potential, while variation of speed was obtained by modifications of field windings.

Meanwhile, commercial work had begun in Great Britain, the first installation being the Portrush Electric Railway to the Giant's Causeway in Ireland, installed in 1883 by Siemens Brothers of London under the direction of Dr. John Hopkinson. The power

was generated by a water turbine, and the current transmitted by a third rail carried on posts alongside the track, with track return, the pressure used being about 250 volts. In the same year there was installed a short road at Brighton, using the running rails, and also experiments conducted with storage batteries at Kew Bridge, London.

The year 1885 showed at least equal activities. Van Depoele operated a train pulled by a dummy at the Toronto Exhibition, taking the current from a conduit below the car; and Daft installed small exhibition roads at the Mechanics' Fair, Boston, the Point of Pines and Coney Island. In August Messrs. Bentley and Knight, who had conducted some experiments in the yards of the Brush Electric Company at Cleveland in the previous autumn, installed a conduit system on the tracks of the East Cleveland Railway Company. This equipment covered a section of the road two miles long, and the conduits were of wood laid between the tracks. Two cars were employed, each of which was equipped with a motor carried under the car and transmitting power to the axle by wire cables. The current was supplied from a Brush constant-current, arc-light dynamo. The equipment operated at intervals during the winter and was then abandoned.

In this year, also, Dr. Wellington Adams, of St. Louis, proposed a radical departure in motor mounting. According to this the armature was carried directly on the axles and the field magnets by a frame rigidly secured to the axle-boxes, this frame also carrying an intermediate gear which formed the connection between the armature pinion and the axle gear. In operation the armature and axle revolved in opposite direction. The scheme was found impracticable and was not introduced commercially.

THE KANSAS CITY ROAD.

In 1884–85 another inventor, John C. Henry, installed and operated in Kansas City a railway supplied by two overhead conductors, on each of which travelled a small trolley connected to the car by a flexible cable. The motor was mounted on a frame carried on the car axle, to which the power was transmitted through a clutch, and a nest of change-gears giving five speeds, very much after the fashion of present-day automobile practice. Subsequently equipping a portion of another road, he conducted a number of experiments in some of which the rails were used as a

return. Various collectors were devised, and one is said to have been carried on the car, but the final selection was a trolley with four wheels disposed in a horizontal plane, carried by and gripping the sides of the trolley wire, and pulled along by a flexible cable connecting it to the car. This feature, using one overhead wire and the rail return, was used on a small road installed by Henry at San Diego, California, two years later.

In the early part of 1885 Prof. Sidney Short, of Denver, began a series of experiments on a short piece of track, followed by the construction, in connection with J. W. Nesmith, of a section of road for conduit operation. The series system was used, a constant current being sent through all the motors on the line in series, by automatically sectionalizing the conductors, the total potential varying according to the number and duty of the motors. Speed and direction of movement were varied by shifting the commutator brushes or diverting a part of the current around the motor. The experiments were continued into the following year and were repeated at Columbus, but failed because of the principle involved. Later, adopting multiple distribution and operation, where each motor is independently supplied from a source of constant pressure, Short attempted the use of gearless motors, but soon reverted to the geared type.

FIRST U. S. COMMERCIAL ROAD.

Daft began work on the Hampden Branch of the Baltimore Union Passenger Railway in August of this year, at first with two motors which pulled regular street cars, and doubled his equipment the following year. Current was taken from a centre rail, with track return, except at crossings, where an overhead conductor was installed, the connection to which was made by a transversely hinged arm carried on the car and pressed upward by a spring. Power was transmitted by a pinion connected with an internal gear on one of the axles.

I believe that this was the first regularly operated electric road in the country, and the conditions of the contract required a year's satisfactory operation before payment. It is said that a certain scientist remarked that any one who would undertake such contract was either a fool or a knave, but this was not the only instance in which those who were strong in the faith were willing to take great risks. That this opportunity came to Daft was due

to the progressive instincts and determination of Thomas C. Robbins, an earnest believer in the possibilities of electric traction, whose efforts met with much of indifference and opposition on the part of the owners of the road.

Encouraged by his success in Batimore, Daft soon undertook a more ambitious experiment, the equipment of a two-mile section on the Ninth Avenue Elevated Railroad, New York, where in the summer of 1885 he operated a locomotive called the "Benjamin Franklin," which, pulling a train of cars, made several trips. The motor was mounted on a platform and pivoted at one end, power being communicated to the driving axle through grooved friction gears held in close contact by the weight of the machine reinforced by an adjusting screw. The experiments were soon suspended, and not resumed until three years later, when for several weeks a rebuilt and improved "Benjamin Franklin" was frequently run between the steam trains on the section between Fourteenth and Fiftieth Streets. A speed as high as twenty-five miles per hour was sometimes obtained, and on one test an eight-car train was pulled up the maximum grade of nearly two per cent. at a seven-mile gait. The adoption of a low potential, to which Daft seemed to have then committed himself, militated against higher speeds.

In the summer of 1885 Van Depoele resumed the operation of the train at the Toronto Exhibition, but on this occasion he used an overhead wire carried on brackets, with a weighted arm pressing a contact wheel up against it. His first commercial installation was made in the autumn at South Bend, Indiana, where five small cars were operated, followed by one at Minneapolis, where an electric car took the place of a steam locomotive. In the following year he installed small roads at Windsor, Canada; Appleton, Wisconsin; Ft. Huron, Michigan; Scranton, Pennsylvania; and Montgomery, Alabama, using on the last a non-reversible under-running trolley carried on the forward end of the car, which was run around a loop for return movement, but on some other roads a travelling trolley carried on the ovehead wire and connected to the car by a flexible cable.

Abroad little was being done. Mr. Anthony Reckenzaun, an advocate of storage battery operation, had conducted some tests in 1883 with an electric launch, and then operated cars in 1884–85 at Millwall and Battersea, England, following with a similar demonstration in Berlin at the end of the latter year. His car

body was mounted on bogie trucks, each of which carried a motor connected by worm gearing to one of its axles. Two sets of brushes were used for reversal, and the speed was varied by grouping the motor armature and field circuits, sometimes also by the use of a starting resistance.

A short road was installed at Bessbrook-Newry under the direction of the Messrs. Hopkinson in 1885, and also one at Ryde in 1886, in which year also Holroyd Smith installed the Blackpool road. In this a conduit system was used with a complete metallic circuit. The motor was carried under the car between the axles and connected by chain gearing. Fixed brushes with end contact were used for both directions in running.

SPRAGUE EXPERIMENTS.

When I separated from Mr. Edison in 1884 and formed my own company it was at considerable personal risk. The company was only a paper one, with a nominal $100,000 capital, all issued to me for inventions and patents, and for which I was to bear the cost of all experimental expenses and perform sundry services for a modest salary which I was to pay to myself. A few shares having been sold and the proceeds soon used for personal expenses, I made a verbal contract with Mr. E. H. Johnson, then president of one of the Edison Companies, by means of which he was to advance certain moneys for a specified interest in the company. One small room sufficed for our business requirements, and the stationary motor development was carried on in the shops of Bergman and Company, New York. But in the fall of the year we had made progress enough to be able to send to the Philadelphia Electrical Exposition a number of motors, among others a new constant-speed non-sparking type, which was the forerunner of a rapidly growing business, accentuated by the endorsement in 1885 by the parent Edison Company for its use by their licensees.

ELEVATED RAILROAD EXPERIMENTS.

My mind, however, soon reverted to the railway problem, and having conceived in 1885 a novel scheme of operation for the Manhattan system, this embracing the now universally used "wheelbarrow" method of motor suspension, I elaborated it to some extent in a paper read before the Society of Arts in Boston in December, 1885. In addition to the axle mounting of the

motors, which were shunt wound, resistances were provided in both the armature and field circuits for variation of speed, return of current to the line and for electric braking.

About this time the Edison-Field interests had installed a battery of small dynamos in the old Durant Sugar Refinery on East Twenty-fourth Street, New York, and Field had made a short trial of a locomotive controlled by a water-rheostat on the Thirty-fourth Street Branch of the Elevated, where a third rail had been laid between the tracks. I had already begun the construction of motors to demonstrate the plans described at the Society of Arts, and the Edison-Field activities having come to a halt arrangements were made by Mr. Johnson for the prosecution of my experiments in connection with the same generating equipment.

The first tests were carried on with two motors mounted on an elevated railway truck under a flat-bottom car, on a track about 200 feet long between the walls of the refinery. Among those who came to see these experiments was the late Jay Gould, then one of the principal owners of the Manhattan Elevated, and it is likely that overconfidence on my part was one of the causes of his subsequent lack of interest in electric developments. Mr. Gould was a man of small stature, and during the operation of the car was standing near the controller and an open safety fuse. On account of the short space for operation the controller which governed the motors for both running and braking was handled quite abruptly, with the result that the fuse blew, with a startling flash, followed by Mr. Gould's attempt to jump off the car.

The experiments were later transferred to the Thirty-fourth Street Branch, and in May of 1886 were witnessed there by a large number of officials connected with the elevated and other enterprises, but although the work continued until December I believe no stockholder or director of the Manhattan road apparently took any further interest in the outcome. But the machines used were the parent models of the modern railway motor. They were centered through their brackets, wheelbarrow fashion, on the driving axles and suspended at the free ends by springs from the transoms. They were single geared, had one set of brushes, were without weather protection, and were used not only for propelling the car but for regenerative braking. During the the latter part of the experiments a second pair of motors was added, with

an interpole winding to maintain fixed non-sparking positions for the brushes. Soon afterwards one of these motors was put into service at the East Boston Sugar Refinery, taking current from a trolley carried on an overhead wire, and was later rescued and the winding restored to illustrate the interpole winding which later has become so important. Two others later operated the first electric snow-sweeper and ice-cutter on the Allston division of the West End Railway in Boston in 1888.

Like many other inventors I was apparently ahead of the times, and, changing my plans for the time, on the suggestion of Mr. J. H. Vail I soon began the equipment of a locomotive car, to be equipped with four seventy-five horsepower motors, each with two armatures geared to the axles and with the magnetic fields in tandem as on the present New York Central locomotives. This is still used for experimental purposes in Schenectady.

TURNS TO TROLLEY PROBLEMS.

The Elevated field offering little of immediate promise, I soon turned my attention to the trolley problem.

Reviewing the conditions at this time, eight years after the Berlin Exhibition by Siemens, it appears that, including every kind of equipment, there had been made only about nine installations in Europe and ten in the United States, with an aggregate of about sixty miles of track and less than a hundred motors and motor cars, characterized by the utmost diversity of practice. There were high and low pressures, series and multiple circuits, traffic-rail conductors and conduits, third-rail return, slotted-overhead tubes, single- and double-overhead wires with single- and double-travellers on them, and upward-pressing arms carried on the cars. The motors were of varied construction and control, generally using two sets of brushes for reversal of movement. One motor to a car usually constituted an equipment, being carried on a dummy or on the front platform, and were connected to one axle by a belt or chain drive. The cars were mostly single ended and controlled from one point. The art was in a chaotic state, and commercial success on a large scale, involving radical departures in practice, was needed to focus the advantages of electric traction, even then thrusting themselves into prominence.

Supplementing small additional roads by Van Depoele at St. Catharine's, Ontario; Lima, Ohio; Binghamton, New York; and

Jamaica, Long Island; and by Daft at Asbury Park, New Jersey; and Los Angeles, California, such an opportunity came in the spring of 1887—and, by good fortune, to my company—in the contracts for the Union Passenger Railways of St. Joseph, Missouri; and Richmond, Virginia; and about the same time one of somewhat different character to the Bentley-Knight Company, on the Observatory Hill Passenger Railway of Allegheny City, Pennsylvania. That year may be said to mark the beginning of the modern commercial development.

The latter road was about four miles long, one-quarter being of conduit construction and the remainder having a double trolley line on side poles, with a travelling trolley connected to the car with flexible cables. It presented unusual difficulties. There were thirty-four regular curves and numerous heavy grades, the maximum being over twelve per cent. and one averaging six per cent. for nearly a mile. The cars were equipped with two fifteen horsepower motors geared to the axles and overhung. The control was by resistance variation. This line was opened early in 1888 and continued in successful service for some time; but the conduit was finally abandoned, and a new equipment was installed with under-running trolley.

THE RICHMOND ROAD.

In the capital of the old Confederacy a new one was assaulted, but this time it was one of physical difficulties, adverse financial and operating conditions, and all the ills of a new and untried system. When my company took the contracts for roads at St. Joseph and Richmond we had little to show other than some blueprints, the machines used on the elevated railroad experiments, and some crude machines used in connection with storage battery operation, but confidence was strong and the contracts were taken under terms, price and guarantee easily placing them ordinarily in the "knave or fool" class, especially in view of the unprepared state of the company to undertake a work of such magnitude. I have, however, always had an abiding faith in the necessity of backing one's faith in a new development to the limit of one's capacity, and the need of this undertaking to break through the inertia of doubt and disbelief.

The Richmond contract called for the completion in ninety days of the equipment of a road having about twelve miles of

track, at that time unlaid and with the route only provisionally determined; the construction of a complete steam and electric central-station plant of three-hundred-and-seventy-five horsepower capacity; the construction of an overhead line; and the furnishing of forty cars with eighty motors and all the appurtenances necessary for their operation. This was nearly as many motors as had been installed on all the cars throughout the rest of the world. Thirty cars were to be operated at one time, and grades as steep as eight per cent. were to be mounted. Finally, the payment was to be $110,000—"if satisfactory."

Fortunately for the future of electric railways the difficulties ahead could not be foreseen, otherwise the contract might never have been closed. But disheartening as these were, great as was the expense incurred and grave as were the risks encountered, they were justified by the results, for the Richmond road, by common consent, stands as the prototype in almost every essential detail of the modern electric-trolley system, and its installation marked the real beginning of the great industry of electric traction.

The contract was made in May, 1887, and shortly afterwards I was stricken with typhoid fever, leaving for the time being the burden on my two principal assistants, Lieut. Oscar T. Crosby, a West Point graduate, and Ensign S. Dana Green, of my own Alma Mater, the Naval Academy, both of whom had resigned to enter the electrical field.

Although a hundred and one essential details were undertermined, during my absence everything possible was done, and on my way back from a convalescent trip to the West I had the pleasure of seeing one of my trusted men, David Mason, start a car on the track at St. Joseph, and immediately afterwards I resumed general charge of the work. I had not yet seen the Richmond road, but much of the track-work was finished, poles were set, many of the motors constructed, and experimental work on them and the controlling switches and trolleys was under way.

The construction syndicate was clamorous for operation to begin, excuses on our part were without number. I shall never forget my feelings when, after inspecting the improvised car sheds at one end of the line, I reached the foot of the steepest hill on my return, and faced a grade varying from four to ten per cent. and about a mile long. The condition of the track was simply execrable, built for profit, not for permanence. The flat twenty-seven-

pound tram rails, of antiquated shape, were poorly jointed, unevenly laid and insecurely tied, on a foundation of red clay. The many curves were sharp, some with a twenty-seven-foot radius; they had only one guard rail and spread easily. The car-house was an open lot on which were two roughly covered sheds. Altogether, I began to realize the extreme hazards, both electrical and financial, which had been imposed upon my company.

The history of the Richmond road has been too often written to dwell upon it at any length here. Suffice it to say that after experimental runs in the latter part of 1887 it was put into commercial operation in the beginning of February, 1888, and for a year there followed an experimental period of development which taxed the resources of the company to the limit.

The general features characterizing it may be briefly summarized as follows: A system of distribution by an overhead line carried over the centre of the track, reinforced by a continuous main conductor, in turn supplied at central distributing points by feeders from a constant-potential plant operated at about 450 volts, with reinforced track return. The current was taken from the overhead line, at first by fixed upper-pressure contacts and subsequently by a wheel carried on a pole supported over the centre of the car and having free up-and-down reversible movement. Motors, one to each, were centered on the axles, and geared to them; at first by single and then by double reduction gears, the outer ends being spring supported from the car body, so that the motors were individually free to follow every variation of axle movement and yet maintain at all times a yielding touch upon the gears and absolute parallelism. All the weight of the car was available for traction and the cars could be operated in either direction from either platform. The controlling system was at first by graded resistances, effected by variation of the field coils from series to multiple relations, and series-parallel control of armatures by a separate switch. Motors were run in both directions with fixed brushes, at first laminated ones placed at an angle, later solid metallic ones with radial bearing, and finally carbon, as proposed by Van Depoele.

The well-nigh heartbreaking experiences and the record of good and bad performances are largely matters of personal history, but the results accomplished soon commanded the attention of those interested in street transportation, most prominent among

whom at that time was Henry M. Whitney, President of the West End Railway of Boston, who was considering the adoption of the cable. He consented to come to Richmond, and accompanied by his associates stopped also at Allegheny City to see the underground conduit of the Bentley-Knight Company. The demonstrations made for his benefit were conclusive, the cable proposal was abandoned, and orders given for trial installations of both the overhead and underground systems to run from the Providence depot in Boston to the suburb of Allston.

Richmond's early troubles were buried under an immediate financial loss to my company of about $75,000, fully compensated for, however, in the subsequent growth of a great industry.

TELEPHONE TROUBLES AND THE BOOM.

To enliven the general situation, however, no sooner was the single overhead trolley with track return adopted than there began a battle with the telephone companies, who claimed the earth and all therein, which was fought to a legal and technical finish in twenty-seven States of the Union, as well as in England. The troubles which they were already experiencing because of the use of grounded circuits were intensified wherever there was an electric railway, partly because of sympathetic induction and partly because of derived circuits, which made the hissing and frying noises intolerable, to say nothing of frequent burn-outs of telephone circuits. Hence the trouble, but in the end we remained below, and they for their own salvation went up higher and adopted complete metallic circuits, for which improvement the electric-railroad men are entitled to the gratitude of the community.

Franklin Leonard Pope, in a historical sketch read before the Electric Club in 1891, referred to this pioneer enterprise in the following words:

> Laboring under enormous difficulties and drawbacks Sprague succeeded, by the completion and operation of this (Richmond) plant in establishing beyond peradventure the future supremacy of the electric street railway, and many of the characteristic features at that time designed and introduced by him have practically become standards in the modern system, and are found in nearly every one of the thousands of cars now in service.

OFFERED THE VAN DEPOELE COMPANY.

During the progress of the installation at Richmond the Van Depoele Company was offered to me by William J. Clark; but partly because of confidence in my own work and lack of appreciation of Van Depoele's, to say nothing of the fact that our resources were already taxed to the limit, the offer was not long considered, and shortly afterward it fell into the hands of the Thomson-Houston Company, which had only a short time previously entered the railway field.

The final success of the Richmond road, the rapid equipment of a number of others, and especially the adoption of electricity on the West End Road of Boston by Mr. Whitney, whose first installation was part conduit and part trolley, and to whom must be awarded the credit for initiating the modern consolidation of street railways, were followed by a period of extraordinary activity in commercial and technical developments, in which for a time the Sprague and Thomson-Houston companies were the principal competitors, more than two hundred roads being put in operation or under contract within two years. The contest between the companies was of the keenest sort and the methods of exploitation most varied; but in the general progress they were aided by the rapidly growing interest and belief in electric railways and the pressure exerted in many communities to abandon old practices. An amusing illustration is offered by the call for a mass-meeting held in New Orleans to demand that a line operated by mule power should change to the new system. The heading of the call, here reproduced, is typical of the South.

LINCOLN SET THE NEGROES FREE!
SPRAGUE HAS SET THE MULE FREE!

THE LONG-EARED MULE NO MORE SHALL ADORN OUR STREETS.

The progress made in the United States soon commanded the attention of the Old World, and work was begun along the same lines in Italy, where I installed the first road, the Florence-Fiesole, in 1889. A few days after the opening a car jumped a curve, killed and wounded about twenty people, and we were finally permitted to resume operation after installing a short-circuiting switch which could be operated by the conductor, thus permitting electric braking by reversal of the controller. The

first road in Germany was installed at Halle, by our agents, the Allgemeine Elektricitäts Gesellschaft, but it was not until a number of years later that there was any general adoption of the electric railway in the more conservative countries.

Meanwhile, the Sprague Company was absorbed in 1890 by the Edison-General Electric; and soon afterwards, embittered by personal experiences in an attempted suppression of my name and a suggested abandonment of the trollley in favor of the use of low-potential rails, and the proposed creation of a new motor, all failures, I severed my connection with it. The Edison Company was later combined with the Thomson-Houston Company in the General Electric. The Westinghouse Company had meanwhile actively entered the field, and for a number of years these great companies have done the larger part of the electric railway work in this country.

RECORD OF DEVELOPMENT.

The record of the succeeding years is largely that of an extraordinary industrial development, with continuous improvement in, and increase in size of, the apparatus. Form-wound armatures, proposed by Eickenmeyer, replaced irregular windings, and metallic brushes had given way to carbon, this single change, initiated by Van Depoele in 1888–89, going a long way toward making the art a success. Cast and wrought iron yielded to steel, two-pole motors to four, double-reduction gears to single, and open motors to closed ones protected by their own castings.

The horse and cable virtually disappeared on old lines, and new ones in great number were created, the overhead-trolley system being almost universally adopted. The conduit system used on a portion of the Allegheny and Boston lines had been abandoned; and although in 1893 a short line was tried in Washington on the Love plan it was not until the following year that work was begun in New York on the Lenox Avenue line, and finally carried to that successful conclusion which warranted its widespread adoption in that city under the auspices of William C. Whitney and Herbert H. Vreeland, and shortly afterwards in Washington under Connett. Abroad, a Siemens road had been in operation at Budapest since 1889. On account of the necessarily heavy cost of construction street-railway managers would not undertake any such

investment except under most favorable traffic conditions, and only when impelled by prohibition of the use of overhead wires.

INCREASE OF POTENTIAL.

Richmond and the many roads that were contracted for during the next few years were all operated on the system of direct supply, that is, by direct current without the intermediary of substations, and at voltages not exceeding 600 volts. While this was sufficient to meet the requirements of the times, in the very early days I realized the economic necessity of raising the working potential, especially if the matter of general railway electrification was to be attempted. This conclusion was illustrated in the potentials specified as required in a project to electrify the New York and Philadelphia Division of the Pennsylvania Railroad, as outlined in a paper read by me before the National Electric Light Association at Kansas in 1890.

Prior, however, to any serious consideration of this class of operation there began a rapid introduction of interurban railways, which was soon aided by applying the developments which had been made in polyphase transmission by Ferraris and Tesla, and in transformers and rotaries by Stanley, Bradley and others, which made possible the economies of alternating current transmission at high potentials and the advantages of direct-current motor operation, through the intermediary of step-down transformers and rotaries or motor-generators, all vital to long-distance and high-powered work where direct-current motors are used.

LOCOMOTIVES APPEAR.

Soon after the use of electricity for single cars had proved successful, heavier operations were naturally attempted, and as early as November, 1890, a line on the South London Road which was originally designed for cable was opened, the trains being pulled by electric locomotives equipped with a pair of gearless motors having armatures mounted on the axles of the drivers. Two years later the Liverpool overhead trolley was put in operation. Here the trains were composed of two-car units, each car having one motor, the two being operated by hand control. In the spring of the same year, 1893, the Intramural Railway was constructed at the World's Fair, Chicago, where motor cars with

hand control were used to pull trail-cars, and a third-rail supply with track return was adopted.

In 1895 the Metropolitan West Side Elevated Road in that city was equipped on the same general plan. In the following year the Nantasket Beach Road, a branch of the New York and New Haven Railway, was put in operation, and in September the Lake Street Elevated of Chicago. Soon afterwards electric service was instituted on the Brooklyn Bridge, motor-cars being used to handle the trains, at first at the terminals and later across the bridge.

There were few attempts, however, to replace steam operation, and only occasionally were electric locomotives used for some special reason. Among the earlier ones in the United States was one of a thousand horsepower, designed by the firm of Sprague, Duncan and Hutchinson. This was provided with a pilot-lever pneumatic control, and was built in 1892–94 for Mr. Henry Villard, for experimental operation on a line out of Chicago which was never undertaken. A still larger locomotive was built by the General Electric Company and used for operating the trains in the Baltimore and Ohio tunnel in 1895.

THE MULTIPLE-UNIT SYSTEM.

Meanwhile, although for the time being out of active railway work, having taken up the problem of vertical transportation, that of electric elevators, I was keenly interested in the rapid-transit problem, and urgently advocated a four-track, underground, electric railway for New York. To silence the objections of a portion of the daily press, in February, 1891, I offered under heavy forfeiture to install, in four months, on the elevated road a train to be operated by a locomotive car, also one to be operated by motors under the cars with a pilot control, and to make an express speed of forty miles an hour.

All proposals which followed steam precedents seemed a pitiful falling short of the possibilities of electric-train operation. In the development of electric elevators I had adopted distant control of the main motor-controller from a master switch. On the first large contract, that for the Postal Telegraph Building in New York, I had provided for the supplemental control and operation of any elevator in the plant from a single master switch in the basement, with the resultant possibility of simultaneous movement of all of them from one switch.

While pondering over the elevated electric-railway train problem one day the thought suddenly flashed upon me: Why not apply the same principle to train operation? That is, make up trains at any length by the combination of car units, a part or all of them equipped with motors, but all equipped with train lines, without regard to number, end relation or sequence, and to control such trains from either end of any car by a master switch connected to the common train line.

The idea, sketched on a scrap of paper, marked the complete birth of this new method, then named and now everywhere known as the "multiple-unit system." Its great possibilities instantly absorbed my interest, for I saw the opening of a new epoch in electric-railway operation. Here was a way to give a train of any length all the characteristics of a single car, with every facility of operation demanded by the most exacting conditions of service and capacity.

FIRST EQUIPMENT IN CHICAGO.

After two abortive attempts to get the privilege to demonstrate the advantages of the system at my own expense on the Manhattan road in New York an unexpected opportunity suddenly arose in the spring of 1897, when I was requested to act as the consulting engineer of the South Side Elevated Railway of Chicago. A brief inspection of the layout showed a field ripe for multiple-unit application, which I briefly explained to Sargent and Lundy, the engineers, and to Mr. Clark of the General Electric Company, fortunately all old friends. I hastily drew up a report, the main feature of which was an argument in favor of the abandonment of locomotive cars and the adoption of individual equipment under common control—in short, the multiple-unit system. As evidence of my confidence I supplemented the report by an offer to personally undertake the equipment on the general plan outlined, which met with the indorsement of the engineers. The contract was not concluded until after I left for Europe, and then only after a bitter contest with various companies and under most onerous conditions, supplemented by a $100,000 bond for performance.

HARD CONDITIONS IMPOSED.

Among other things I was immediately to begin work on the entire equipment and to have six cars ready for operation, in two

months, on a standard track supplied by me, the manner of making the test to be prescribed by the officers and engineers of the road and to be to their satisfaction. Should the test not be concluded by the date set, or prove unsatisfactory, the contract could be cancelled. Further tests could be called for elsewhere and the remaining equipments were to be completed by specified dates. As soon as the power-house and road were ready there was to be another test of not less than twenty equipments under service conditions for a period of not less than ten days. Should these equipments prove unsatisfactory the right still remained for the railroad company to cancel the contract and to require waiver of all claims.

I did not return to New York until about the middle of June, so that most of my instructions for the trial equipments were by cable, and the actual preparation was made within about thirty days, despite a wholesale strike of the mechanics employed in the shops of the new Sprague Electric Company, which soon took over the contract.

On July 11, 1897, two cars were put into operation on the tracks of the General Electric Company at Schenectady, and on the 26th, the half-century anniversary of Professor Farmer's test of a model electric railway at Dover, my ten-year-old son operated a six-car train in the presence of the officers and engineers of the South Side Elevated Road at Schenectady.

In November a test train of five cars was put in operation in Chicago, and on the 20th of the following April twenty cars, seventeen of which (one in flames) were taken off during the day because of defective rheostats; but with the last three-car train I had the satisfaction of pushing a steam train around a curve. Three months later, a year after the Schenectady test, locomotives had been entirely abandoned and the whole one hundred and twenty cars were in operation.

The controllers for the original Chicago equipment were of the ordinary street-car type, operated by pilot motors automatically retarded by any excess of current in the motors during acceleration. The train line contained three speed- and two direction-controlling wires terminating in couplers at each end of the car. The disposition of the control wires and their connection to the master switches was such that whatever the number, sequence or end relation of the cars there was never any change in the connec-

tion of the speed circuits, while when any car was reversed in make-up position the direction-controlling circuits were automatically reversed. So, also, whatever the grouping of cars, like movement of the master switch with reference to facing the track produced like relative direction of movement. These principles are fundamental whatever the changes of physical details.

As an alternative physical construction the Westinghouse Company first used a step-by-step pneumatic motor to operate the controller, and later, on account of the increase of duty, both the General Electric Company, which finally absorbed the Sprague Company, and the Westinghouse Company replaced the single cylinder form of controller by a combination of individual contactors each under a magnetic blowout, although later there was a return to cylinder operation.

The multiple-unit system is now a fundamental essential, the world over, for all electric-train operation where two or more equipped cars or locomotives are controlled from a common source, and its value in dense rapid-transit service like the Elevated and Subway in New York is indicated by the enormous increase of capacity compared with any other method of operation, a practical result which could not be equalled in any other way on the present subways alone for less than $100,000,000 capital cost of construction.

MAIN LINE ELECTRIFICATION.

Following a serious accident in the yard tunnel of the New York Central road, the first important step in America in main-line electrification was taken when a new station was determined upon, and electricity adopted for operation at and for some distance from the main New York terminal, in 1902–03; and here again there was a radical departure in engineering practice. Up to that time all motors used for railway purposes maintained a fixed relation between the armature and the field, but when this project was finally taken up under the guidance of an Electrical Commission, presided over by the then vice-president, Mr. W. J. Wilgus, and of which commission I was a member, a plan for a new type of locomotive, proposed by Mr. Batchelder, was submitted by the General Electric Company. This called for the use of bi-polar motors, the fields of which, carried in a horizontal plane, were supported by and made an integral part of the locomotive frame,

and hence carried above the suspension springs. The armatures were rigidly secured to the axles, and the fields, with flattened pole pieces and a comparatively large air gap, were free to move vertically relative to the armatures. All brackets and gears were thus dispensed with and the motor reduced to the simplest form. Although ridiculed by many engineers at the time the new design was accepted and has proven entirely satisfactory. These locomotives were the first to be equipped with the multiple-unit control so that two or more could be operated together, the same as the suburban cars. On this equipment was first developed the W. and S. under-contact rail.

In Europe the first important main-line electrification projects were carried out on the polyphase system, with a high degree of technical skill, by the Ganz Company on the Valtellina and the Simplon Tunnel lines. On these the axle mounting was abandoned, and the motors were carried on the locomotive frame, being connected to the drivers by side rods.

ALTERNATING VS. DIRECT-CURRENT MOTORS.

As already stated, in the early days of electric railroading it was apparent that at the electric pressures commonly used, from 450 to 600 volts, the field of operation would be restricted by the large investment required for copper, not of course within ordinary city limits but when the distances became considerable. I had, therefore, advocated improvements looking to the raising of direct-current potentials. For a long time this seemed impracticable to many engineers.

Meanwhile, the system of polyphase alternating-current transmission, with conversion to direct current at substations through the intermediary of step-down transformers and rotary converters or motor generators, had been developed to such an extent that the field of operation from a single central station was materially broadened. Of course, there was the investment at the substations, by no means a small one, and many engineers ardently recommended the abandonment of all consideration of the use of direct current for interurban and trunk-line operation, and urged the adoption for this purpose of either polyphase or single-phase alternating-current motors, with direct supply at high potential on the trolley wire, speed control to be attained by the use of a step-down transformer or otherwise.

Among the engineers who became prominently interested in this technical development may be mentioned Lamme, Finze, Winter, Eichberg and Steinmetz. It was conducted along two or three different lines. One type, originally proposed by Thomson and known as the "repulsion" motor, had the field supplied direct at the full-trolley potential. This has been largely abandoned, although an alternative of this form was developed by European engineers, in which latter type a variable potential was supplied to the armature from a transformer.

Another, and more successful type, is the series-commutator motor, much like the ordinary direct-current motor, except that the iron in the magnets is laminated to prevent loss and the pole pieces have an additional compensating winding across their faces. The high-tension current from the trolley is transformed to low tension on the locomotive and speed regulation is obtained by the use of the rheostats or voltage variations on transformers. Naturally, there are many variations in detail introduced on the equipments actually installed for railway service.

The great difference of opinion among American engineers and manufacturers gave rise to bitter controversies, which rose to a climax at the time of the adoption of the single-phase series-commutator type of motor on the New Haven road, whose trains had likewise to operate over a considerable section of the New York Central tracks at their common terminal.

THE INTERPOLE MOTOR.

Having been in the forefront of this controversy, the outcome has been of special interest. During a long period of doubt among many as to the results of single-phase operation my attention was called to developments which had taken place in variable-speed, direct-current motors for ordinary industrial purposes by making use of my old interpole winding, localized on small extra poles carried between the main poles of the motors, and in consequence I urged a test of this practice on railway motors. The first results attained were so remarkable that I instantly saw the possibilities of a great increase in the potential which could be used in direct-current operation, and that if this improvement was carried to a logical conclusion the probabilities were that the economical requirements of the larger problems could be met by this system. On account of certain inherent defects in the single-

phase motor it also seemed likely that it would gradually be abandoned, and since its sole claim for use had been based upon the economy of installation and transmission direct current might maintain its supremacy not alone upon urban and interurban roads but also in trunk-line operation.

There was, indeed, the specific lack of capacity inherent to a single-phase motor, to combat which increased armature speeds were supplemented by air-blast cooling. But of course these were equally applicable to the more sturdy direct-current motor, and were soon adapted for it, so that the relative capacities for equal weight efficiencies and heating remains as originally indicated, at least where twenty-five cycles are used.

Despite this general fact the single-phase system, which attained an early vogue in Switzerland, has continued to receive the support of the engineers of that country in its use on the State Railways, generally with 15,000-volt trolley supply, but at the low frequency of fifteen cycles.

Lack of relative motor capacity does not characterize the polyphase motor, and it has received the support of Italian engineers in several of their major installations, comparatively low trolley potentials and low frequencies being used.

In the United States a notable example of use is that on the Great Northern Railways. One of the most ambitious attempts on this plan was that undertaken by two German companies, many years ago, on the Zossen Military Line, where was made the highest record for speed of a car carrying passengers—about 126 miles per hour—the current being collected at 10,000 volts from three overhead wires by sliding contact.

The multiplicity of conductors, however, distinctly militates against the polyphase trolley system as a solution of the larger railway problem, despite the high ratio of motive power to weight and the easy use of the motor for braking by regeneration, quite independently of other limitations affecting trunk-line transportation in general and the supply of power for such from central power plants as are also used for the industrial supply of light and power.

Additional methods of using single-phase alternating currents on the working conductor have been proposed and put into practice, but these eliminate the single-phase motors entirely. Among them is the introduction on the locomotive of apparatus for changing the energy of the single-phase trolley current into polyphase cur-

rent to be used in polyphase motors, as on the Norfolk and Western Railroad.

Conversion into a pulsating direct current by the use of a mercury rectifier carried on the locomotive has been tried out quite extensively on experimental equipment, and also the use of mercury rectifiers operated from polyphase currents at substations, in both cases in connection with direct-current motors.

On the whole, the experience of the past twenty years seems to have demonstrated the soundness of the conclusions advanced with regard to direct-current development, for while the New Haven system has been necessarily maintained and extended, many of the other single-phase motor installations in the country have been abandoned, and on the other hand some of the most difficult and extended freight lines, at one time deemed by many engineers barred to direct-current operation, have adopted direct current at from 2400 to 3000 volts working potential, as illustrated by the Butte and Montana Railroad and the six hundred miles on the Chicago, Milwaukee and St. Paul Railway, this latter being the most ambitious project thus far undertaken. Following in general a plan suggested in a study of electrification for the Sacramento Division of the Southern Pacific Railroad some years ago, the St. Paul uses the motors for regeneration and braking on down grades.

Nor is the vogue of the direct-current operation confined to the United States, for French and English commissions have reported in favor of its general adoption, and on the Midi Railway of France single-phase operation has been abandoned in favor of direct-current motor operation, the conversion of A. C. to D. C. current at the substations to be effected by large capacity mercury rectifiers.

Incidentally, the development of the automatic substation, with material elimination of attendance, and a general raising of line potential and consequent efficiency by a more frequent distribution of smaller conversion units, will add to the extent and flexibility of the direct-current system.

Finally, a recognition that railroads are but one of many diversified users of electric power, and that for financial and economic reasons common sources of supply should be utilized, will prove influential in determining that system which best lends itself

to the requirements in the matter of frequencies of the most economical high tension transmission.

THE FUTURE.

It has been said that the throb of the locomotive was the heart-beat of civilization, and so the hum of the motor may well be considered a song of emancipation. Every country with expanding resources owes its growth and prosperity largely to the development of its arteries for transportation, and its future will in a large measure depend upon the efficiency with which such continues to respond to the demands made upon them.

It has been predicted that, with normal growth, traffic requirements in the United States will be doubled well within a score of years, but already many of the trunk lines are within sight of the limit of capacity at terminals and portions of the rights of way, under existing conditions of operation. The transportation problem, therefore, promises to be one of the most serious which confronts railway and governmental execution.

Increase of capacity is the coming vital need, to be achieved where possible along existing lines and by existing methods, but where these fail then in the adoption of a motive power which permits material or even radical changes in train make-ups and operation, and increase of capacity.

The steam locomotive and its rival cannot be compared simply as machines, for there are inherent differences between them. However improved, the former remains a moving power plant, limited both as to its maximum and continuous rate of power development by the capacity of its boiler and a portable fuel supply. It is an independent entity, which fact has certain advantages but also certain disadvantages.

The electric locomotive, on the other hand, is a transformer of electric energy created at distant power stations and transmitted to it by stationary conductors. Thanks to the multiple-unit system any desired concentration of power units under a common control is possible, and any number of power plants, taking their energy from centuries-old and diminishing supplies, of increasing cost, or the annually renewed white coal of the mountains, can be joined together in common supply.

Already it has been conclusively demonstrated that with the electric locomotive any given mountain traffic can be handled with

a fewer number of units and at higher speeds; that it is capable of longer hours of continuous service under adverse weather conditions and with less terminal inspection and repairs; that train schedules can be increased not only because of the superior speed on heavy grades but because of the elimination of waits for coal and water supplies, with fewer lay-overs at a reduced number of passing points on single-track railroads; that there may be a material reduction of useless dead-tonnage haul; and that if there were universal electrification there could be saved at least one-half of the coal now used on railroads, which already amounts to a quarter of the total mined.

Among other outstanding facts it has also been established that any required amount of power can be transmitted over long distances with entire reliability and with limited losses; that it can be stepped down from very high to any required potentials at local centres of supply; that it can be distributed thence in the form of single or polyphase alternating, or direct current, the former by overhead conductors and the latter by both overhead and third-rail iron conductors; and that it can then be used in any form desired in the motive-power equipment—in short, that electricity itself, now a source of power and its character still unknown, is at once the most docile, tractable and universal servant of man.

But despite the enormous advances made and the results already accomplished it would be folly for the electrical engineer or the railroad man to assume that the limits of invention or development had been reached, as evidenced by the steady march of improvement to meet new problems and recent remarkable prophecies of things to come.

The urban and interurban fields, with the constant linking up of smaller systems into more extended systems, has gone on apace, but the trunk-line systems are still largely steam operated, and there are wide differences of opinion among engineers as to whether a single system will be dominant, and if so, what one, or whether the varying conditions and demands will be best met by specific solutions.

The financial question involved in the large cost of equipment cannot but remain a factor which will often prove controlling, for electrical operation will not be generally adopted except there be a commensurate gain of some kind. Where coal at low unit

cost is the source of power the gain in economy alone will not warrant the adoption of electricity on independently operated roads, but where coal is high in price or water power can be gotten at a reasonable cost then there is a valid reason for the change.

Excluding special cases, what will ultimately be constructively influential will be that need of increase in existing or available track capacity which I have already indicated, which is undoubtedly possible to a system which permits of individual and simultaneous control of a concentrated or distributed power plant greater than can be gotten by any other means, and can eliminate from its tracks the transportation of its fuel. It seems certain, however, that there must be coöperation in the important matter of power supply, and the general trunk-line problem will appear less formidable with the elimination of the requirements of installation of individual power houses, with their necessary reserves, and the use of current from great power plants properly linked together, which in addition to their reliability can make full use of the diversity factor in a multitude of demands.

Finer Dust Particles in Air.—A. L. MEYER, of Johns Hopkins University (*Jour. Ind. Hygiene,* 1921, iii, 51–56), has devised a simple method for the determination of the number of finer dust particles in air. The sample is collected in a Luer syringe which is graduated to 100 c.c. and has a total capacity of 160 c.c. The count of the particles is made in a Levy blood-counting apparatus. The syringe is thoroughly cleansed, then filled with distilled water free from air bubbles. This water is forced completely out, and 20 c.c. of distilled water are drawn into the syringe from a flask which is provided with a filter of cotton wool. A sample of air, with a volume of approximately 100 c.c., is now drawn into the syringe, and its exact volume is read from the graduations. The nozzle of the syringe is covered with a rubber membrane which is held tightly in place. After shaking vigorously for one minute with an up-and-down motion, the syringe is held in a vertical position; and the water is forced to the very tip of the nozzle, then withdrawn so that a small bubble of air is admitted. Any particles of dust which have adhered to the narrow region of the nozzle are also brought into suspension by the latter manipulation. The metal connecting piece is attached to the syringe; and one drop of the liquid is permitted to flow into the chamber of the counting apparatus. After the particles have settled, count is made of the number present in two fields, each one square millimetre in area. J. S. H.

SOME APPLICATIONS OF PHYSICS TO ORDNANCE PROBLEMS.*

BY

GORDON F. HULL, Ph.D.

Technical Staff, Office of the Chief of Ordnance, War Department, Washington, D. C.

IN a very recent number of one of our most popular magazines, founded, by the way, nearly two hundred years ago by the scientist and philosopher whose name this institution bears, Ex-Secretary Daniels gives three reasons for building dreadnoughts—they are first, gun power; second, gun power; third, gun power. In this statement Secretary Daniels confirms our view that a battleship is merely a gun carriage. The submarine is a special form of disappearing gun carriage. Were it not for the guns on board, the battleship and the submarine would have no reason for their existence.

It is the business of guns to throw projectiles. The only reason, therefore, for putting $635,000,000 into battleships now being built and to be built during the next three years, is that they may throw more projectiles, throw them farther and with greater accuracy than can their adversary. In olden times a "Man-of-War" could come to close terms with an adversary. Its fighting men could even board an enemy's vessel and, by cutless and pistol, fight red-blooded battles. Not so in these days. In place of trying to close in on an adversary the modern battleship seeks safety in distance—a distance secure from the enemy's guns, but sufficient for its own guns to work destruction. During the World War the chief naval engagements were decided by these conditions. In future wars the ranges will be greater. The accuracy must be correspondingly increased.

When we consider the vast amount of money which is expended in guns and projectiles, we feel justified in devoting time and energy to a study of scientific problems connected with their functions. My lecture this evening will deal with some experiments which have been and are still being carried on by the Ordnance Department and the Bureau of Standards—experiments having for their purpose the determination of (*a*) the forces ex-

* Presented at a meeting of the Mechanical and Engineering Section held Thursday, March 31, 1921.

erted on a projectile by a rapidly moving air stream, (*b*) the retardation of a projectile as a function of the many variables upon which it depends, (c) the pressure of the explosion in a gun and its rate of variation with time, and (*d*) the expansion of a gun during firing.

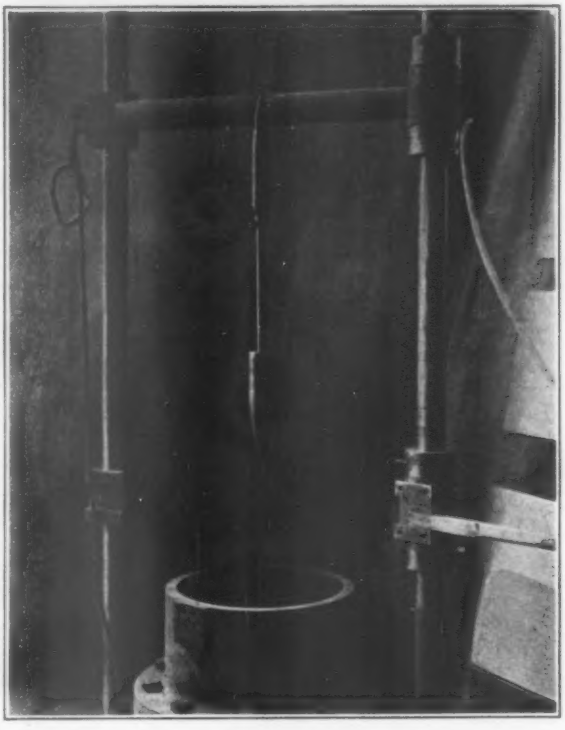
What are the factors determining the flight of a projectile? Obviously they are the velocity, angle of elevation, weight of projectile, and resistance of the air. If there were no air resistance, all projectiles would have the same range and time of flight, given the same muzzle velocity and angle of elevation. The .30-calibre rifle bullet, for example, would have a range of 43 miles and would have at the end of its flight the same energy and destructive action as at the beginning. But the *resistance* of the air, which is a very comprehensive term involving the shape, distribution of mass, spin and velocity of the projectile, as well as the density and temperature of the air, robs this small projectile of nine-tenths of its range.

Before the World War we had contributed nothing to our knowledge of the resistance of the air for projectiles. The law of air resistance, upon which are computed all our range tables both for the army and navy, is taken from foreign sources. In determining this law the classical method of firing through screens was used. As will be shown later, this is a method which has not been carried to great precision, and which as conducted in the past can lead to very incomplete results.

Without going into the history of the matter, we will consider first the method which has been used during the past two years for determining the forces exerted on a projectile by a rapidly moving air stream. Just a word as to the power required to produce the necessary air stream. A wind of 60 miles per hour, nearly 90 feet per second, is regarded as a gale. Power goes up with the cube of the velocity. To produce an air stream of 600 miles per hour or 900 feet per second would require 1000 times the power called for to give the 60-mile gale. To produce a speed of 1500 feet per second, we would have to increase the power 4600 times. It can easily be computed that the power required to produce a one-foot diameter wind stream of a velocity of 1200 feet per second, if we assume that 30 per cent. takes the form of thermal energy and only 70 per cent. takes the form of the desired kinetic energy, is 8600 H. P., or

6300 kilowatts. If this had to be paid for at an electric light rate of 10 cents per kilowatt hour you can see that the cost would be more than ten dollars a minute. Of course we do not have to pay such a rate, and fortunately, through the courtesy of the General Electric Company, we have been able to utilize the commercial

FIG. 1.

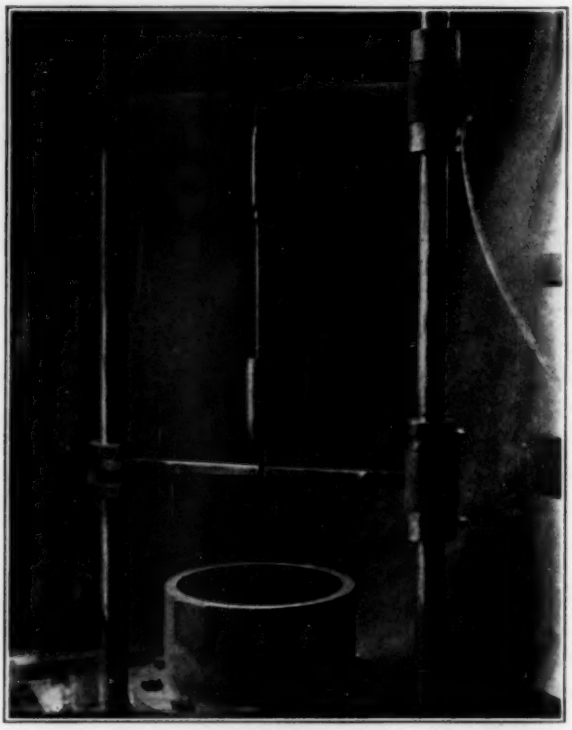


tests of the compressors so that the Government has been called upon to pay for only a small part of the power it has used.

The apparatus now used for measuring the forces on the projectile is illustrated in Fig. 1. The projectile is attached by a steel spindle to a steel piston working in a cylinder, the latter being attached to a horizontal bronze bar which can be moved in and out, or up and down, in the wind stream. The pressure of the air above the piston can be regulated and measured. Hence, when the up-

ward force of the air on the projectile is equal to the weight of the moving system, the piston moves up or down in the cylinder, or a "balance" is obtained. As far as this "weighing" is concerned, the accuracy depends upon the precision of fit of piston and cyl-

FIG. 2.



inder. This precision is such that although only lubricating oil is used between the surfaces, air does not leak in or out to any appreciable extent. At the same time, a difference of force of 0.04 pound will cause the piston to change from an up to a down motion, or vice versa.[1]

[1] Under the direction of Dr. L. J. Briggs, of the Bureau of Standards, who is collaborating in these experiments, these cylinders and pistons were made by Mr. W. H. Cottrell, to whom is due great credit for excellent workmanship.

But the projectile is held by a spindle upon which the moving air stream exerts some force. In order to take account of this force, the projectile is attached at its nose to a small bar as shown in Fig. 2. The spindle, free to move vertically, is placed immediately behind the projectile. The force on the spindle and piston can then be found.

A hollow cylinder can replace a solid one. We can then measure the impact pressure in the air stream, or, mounting the projectile as in Fig. 2, we can measure the pressure at the base and across the base of a projectile. In this way we can compute the force due to the base and the portion that this is of the total.

The velocity of the air and its temperature, after it has issued from the orifice, are computed by the use of the ordinary formula holding for adiabatic expansion, *viz.*,

$$V^2 = 2\,J\,C_p\,K_i\left\{1 - \left(\frac{po}{pi}\right)^{\frac{Y-1}{Y}}\right\} = 2\,J\,C_p\,(Ki - K_o)$$

where

V = velocity of air in metres per sec.
J = mechanical equivalent of heat.
= 4.18×10^7 ergs/gram calorie.
C_p = specific heat of air at constant pressure = .240.
Y = ratio of specific heats = 1.405.
K_i = temperature of air in pipe (Kelvin).
K_o = temperature of air in jet.
P_i = pressure inside of pipe.
P_o = pressure in jet = barometric pressure.

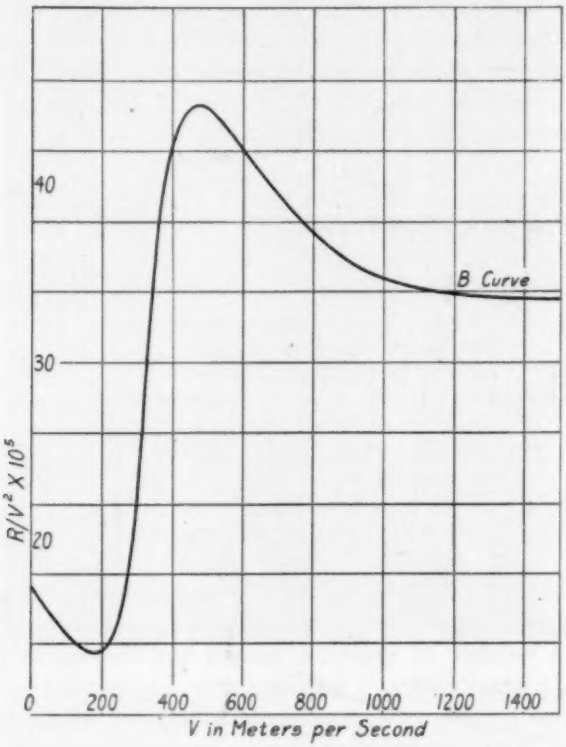
It is necessary to measure the pressure and temperature of the air in the pipe below the orifice, *i.e.*, before expansion. For this purpose a number of pressure gauges and thermo-elements are used. A potentiometer, a galvanometer (suspended so as to be undisturbed by the very pronounced vibrations of the building), and an air pump capable of producing suction or pressure, are also necessary. Photographs of the air waves about the projectiles, especially at high velocity, give an indication of the disturbing causes and also provide a check upon the computed air velocity.

There are a great many variables in this experiment, and there are a great many corrections to be applied and standardizations to be made. But finally we can say that we know the force which an

air stream of a certain velocity, density, and temperature will exert on a projectile of a certain specified form. For a number of definite forms we know the law of air resistance between the ranges of velocity so far used, *i.e.*, from 200 to 380 metres per second (about 600 to 1200 feet per second). And we know the effect upon the resistance of adding a rotating band to the projectile fore or aft.

But it may be argued that we must have known the law of air

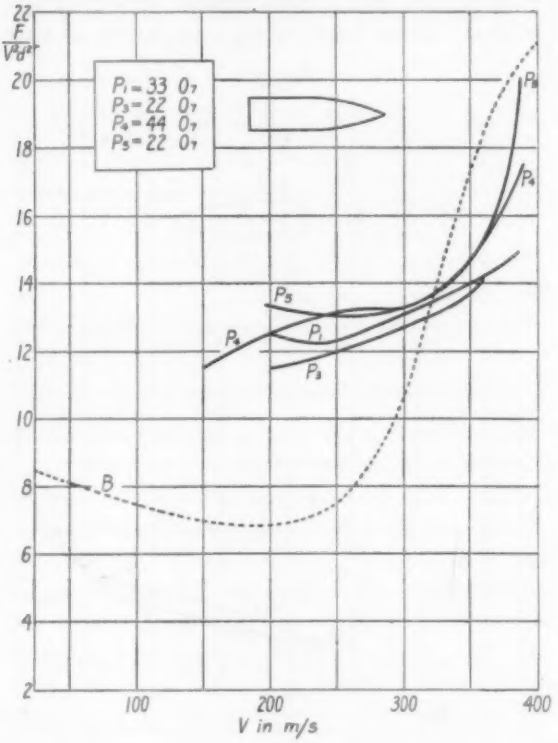
FIG. 3.



resistance for years, otherwise we could not have constructed range tables. It is true that we follow the French law in this work (we have never determined any law whatever for ourselves), but it will be seen in Figs. 4, 5, 6 and 7 that that law does not hold even approximately for modern projectiles, within the limits of the velocity we have used. The French write the law in the form

$R = c\,\rho\,d^2\,V^2\,B(V)$. Hence $R/V^2 = c\,\rho\,d^2\,B(V)$ Where $B(V)$ is a function of V which when plotted has the form of a dotted line in Figs. 4, 5, 6 and 7, and the full line in Fig. 3.[2] Now, projectiles

FIG. 4.



illustrated in Figs. 4, 5, 6 and 7 may be regarded as modern, and

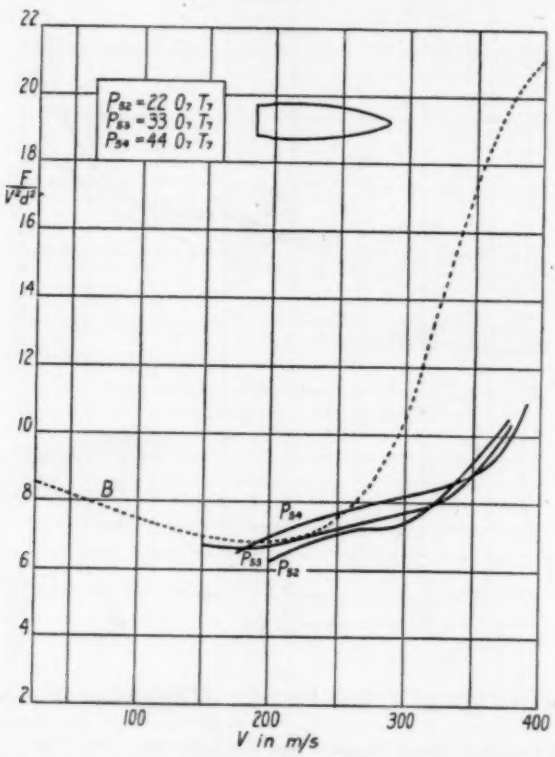
[2] The law of dimensions requires that the force take the form

$$R = \rho\, v^2\, l^2\, f\left(\frac{\mu\, v\, l}{\rho} \quad \frac{v}{c}\right)$$

for any given form of projectile which has a linear dimension l, moving with velocity v in a medium of density ρ, viscosity μ, and compressional wave velocity c (velocity of sound). Here f is an unknown function which may be represented by $R/\rho\, v^2 l^2$. The B curve is this function for the projectiles used by the French in its determination. Its rapid rise in the neighborhood of a velocity of 350 metres per second indicates the influence of the *compresional* term in the dimensional equation.

for these the resistance law departs widely from the B curve—the curve which is the basis of all our ballistic computations. These curves also show that for low velocities (V = 200 to 250 metres per sec.) the force due to the air does not depend very much upon the form of the nose, but that it does depend very critically upon the form of the base. Thus, boat tailing a projectile 9° cuts the force

FIG. 5.



in half for V = 200 m/s. At low velocities the 9° boat tailing is rather better than a 7° or a 5°, but this advantage soon disappears as the velocity increases. Our curves suggest, though they do not in any way prove, that at high velocities the retardation of a boat-tailed projectile is not much less than that for a square base.

It is clear then that when we speak of the "best form" projectile we must specify the velocity for which this term applies.

The important part played by the base of the projectile is fur-

ther made evident by the measurement of the pressure of the air across the base. For a square base projectile, the pressure is fairly constant at all points across the base. This pressure is negative when compared with atmospheric pressure and therefore is suction. The data of Table I show that at low velocities the force due to the

TABLE I.

Data showing pressures of the air at the base of a projectile.

Projectile No. 3, square base. 7 calibre ogive.

V m/s.	Suction at base in cms./Hg.	F_o= Force due to suction at base.	F= Total force.	Ratio F_o/F.
198	2.3	1.53	1.99	.77
230	3.2	2.19	2.52	.80
285	5.3	3.51	4.00	.87
298	5.3	3.40	4.40	.80
322	5.5	3.65	5.40	.70

base is four-fifths of the total force for a 7-calibre ogive projectile. If the area of the base is decreased by boat tailing, and if, as our measurements show, the suction also decreases, then the force due to the base is greatly decreased and the total force on the projectile is similarly affected. Table I shows the suction in centimetres of mercury at the base of a projectile. To interpret this suction in pounds per square inch, one has to divide the number of centimetres of mercury by 5.17.

The data shown in Figs. 4, 5, 6 and 7 were obtained without any rotating band on the projectile. The addition of a rotating band may greatly increase the force or may have no effect, depending upon its position. For a square base projectile, a band flush with the base increases the force by from 10 to 15 per cent. But if the band is placed slightly away from the base there is no appreciable increase over the case of no band. For a boat-tailed base, placing the band at the beginning of the bose cone increases the force from 20 to 50 per cent. In the case of a long-coned ogive, a band placed at the end of the ogive may produce an increase in force of over 50 per cent., while two bands at the end of the base and nose cones may produce an increase of 100 per cent. Clearly, then, small rregularities in the form of a projectile may bring about a surprisingly large increase in the retardation. Table II shows

some of the variations of force due to the positions of the rotating band.

TABLE II.

Variation in force due to change in position of rotating band. Distances of the band measured from the base.

Projectile No. 5, square base. 2-inch dia., 7 calibre ogive.

V = 215 m/s			V = 235 m/s			V = 280 m/s		
Position of band	*force* / V^2	Ratio	Position of band	*force* / V^2	Ratio	Position of band	*force* / V^2	Ratio
no band	530	1.00	no band	520	1.00	no band	535	1.00
0	580	1.10	0	590	1.13	0	620	1.16
.2 inch	520	0.98	.2 inch	534	1.03	.2 inch	570	1.07
.3 "	530	1.00	.4 "	520	1.00	.4 "	540	1.01
.5 "	540	1.02	.6 "	535	1.03	1.0 "	560	1.05

Proj. No. 52 2-inch dia. 7 calibre ogive 7° boat tailed

V = 250 m/s			V = 280 m/s		
Position of band	*force* / V^2	Ratio	Position of band	*force* / V^2	Ratio
no band	29	1.00	no band	30	1.00
1.00 inch	37	1.26	1.00 inch	40	1.33
1.25 "	35	1.20	1.5 "	37	1.23
			2.0 "	38	1.26

Projectile with coned ogive (20° total vertical angle).

V = 280 m/s			V = 260 m/s			V = 280 m/s		
Proj. No. 23 5° boat tail			Proj. No. 29 7° boat tail			Proj. No. 35 9° boat tail		
Position of band	*force* / V^2	Ratio	Position of band	*force* / V^2	Ratio	Position of band	*force* / V^2	Ratio
no band	394	1.00	no band	326	1.00	no band	283	1.00
1.0 inch	432	1.10	1.0 inch	413	1.27	1.0 inch	434	1.53
1.5 "	418	1.06	1.5 "	370	1.13	1.5 "	320	1.13
2.0 "	418	1.06	2.0 "	378	1.16	2.0 "	320	1.13
at ogive	516	1.31	at ogive	562	1.72	at ogive	450	1.59
						band at each cone...	580	2.05
						two bands midway between cones...	332	1.17

The forces platted in the curves here shown were measured when the projectile was held nose-on to the air. There have been indications that when the projectile was inclined to the air stream,

the force was greatly increased. For example, in one case, when a projectile weighing nearly fifty pounds was in the air stream, the nose-on force was a little over forty pounds. There was then a down force on the spindle. But as the projectile was being taken out of the air stream it was set swinging slightly. The air caught it on one of its swings and threw it up several feet, breaking off in that motion the steel spindle holding the projectile to the balance arm. It was evident that the inclined force must have been at least twice as great as the weight of the projectile.

By attaching the projectiles to the spindle at an angle to the axis, the force in the original direction of the air stream may be found for an inclined position. The data in Table III have recently been

TABLE III.

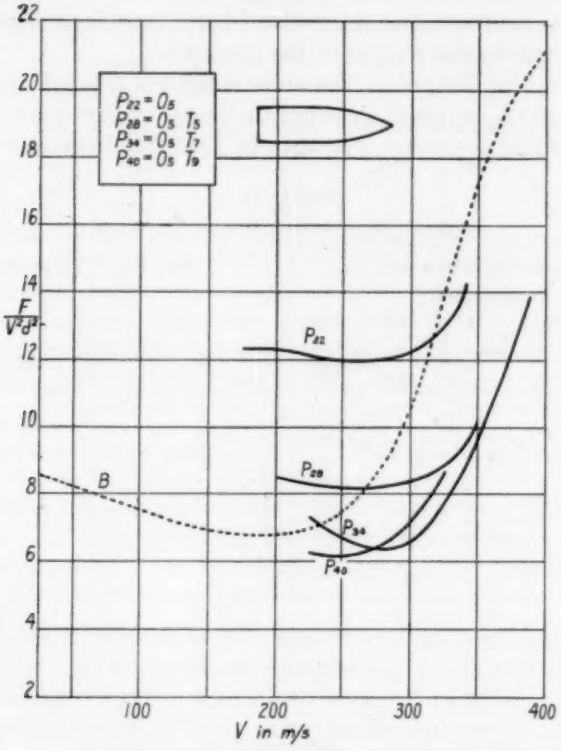
Variation of force due to inclination of projectile.

Proj No. 53 V=200 m/s					Proj. No. 1 V=200 m/s				
0°	2.5°	5°	10°	15°	0°	2.5°	5°	10°	15°
255	287	345	475	660	410	500	530	715	880
Proj. No. 53 V=270 m/s					**Proj. No. 1 V=270 m/s**				
0°	2.5°	5°	10°	15°	0°	2.5°	5°	10°	15°
5.2	5.5	6.3	8.1		8.4	9.8	10.2	14.9	

obtained and may require revision. These measurements show that the retardation of a projectile may be increased two and one-half times by a change in direction of the axis of the projectile through 15 degrees. Now, ballisticians have never taken any account whatever of the change of inclination of the axis of the projectile to the wind. They have regarded the projectile very much as if it were a sphere, *i.e.*, as if the force on the projectile was independent of its orientation. But our data show how serious an error may be made in following that assumption. At high altitudes, the wind velocity may be very large and may easily cause a 15-degree attack of air upon a projectile. In such a case, if the projectile is a stable one, its rapid rotation will cause the nose to precess about the apparent direction of motion of the air, and it may in time meet the air nearly

nose-on, but it may continue to have a large yaw, in which case its retardation will be several times as great as it would have been had it moved nose-on to the air. The experiments with the air stream not only give quantitative results for the forces on projectiles but they also show quantitatively, by means of photographs, the condi-

FIG. 6.

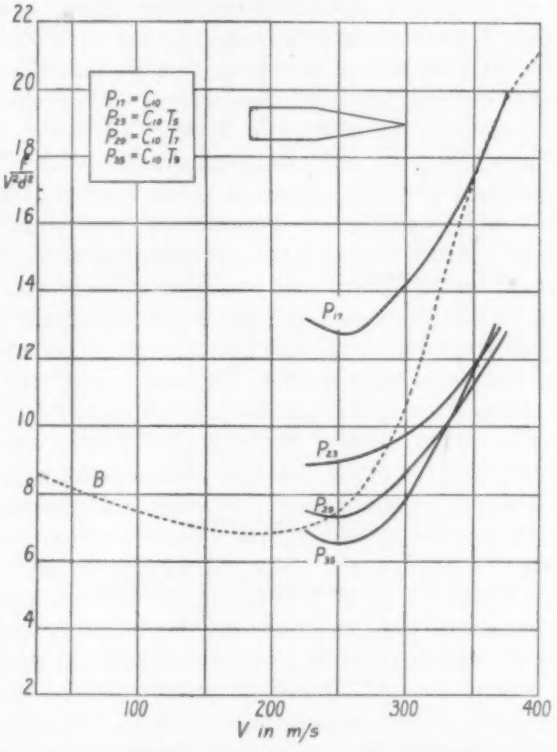


tion of the air round a projectile. (Here the lecture was illustrated with a number of lantern slides of bullets in flight taken by means of an electric spark source, showing the shock waves around bullets of different shapes, and showing how a bullet is forced away from straight line motion when its axis becomes slightly inclined to the nose-on position. Photographs were also shown of the air waves around projectiles in the air stream. These made evident the features of the projectile which were sources of wave motions in

the air and therefore indicated how the form of projectile could be improved.)

The photographic evidence just discussed shows that a projectile becomes inclined to its direction of motion. The wind stream experiments show that for an inclined direction the force on the projectile due to the air is greatly increased. It is necessary then to

FIG. 7.



study the gyroscope action of the shell, the inclination of its axis to its direction of motion, *i.e.*, its yaw, and to the variation of this angle with time. This problem, that is, the problem of the stability of a shell, is being studied by Mr. R. H. Kent, at the Aberdeen Proving Ground.

It is clear from everything that has gone before that the whole story of a projectile is not told by a study of the forces on it as it is

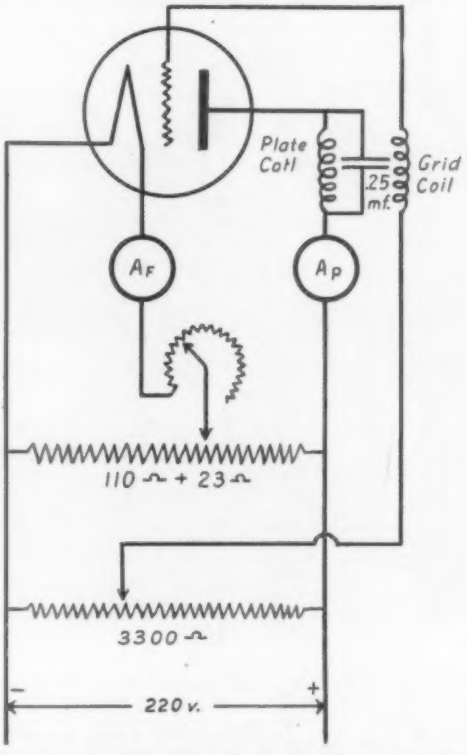
held in an air stream. It is advisable to supplement that information with data obtained from actual firings of a projectile. For this purpose the writer devised a chronograph and a method capable of measuring with great precision the velocity of a projectile at various points along its path.

As has been stated, the method of measuring the velocity of a projectile has been to fire through wire screens, thus breaking a wire and hence an electric current. The breaking of this current dropped a rod or made a signal on a revolving drum. It was clear, however, that the wires might be broken by the air waves in front of the projectile or might not be broken, if at all, until the projectile had passed completely through the screen. Thus the distance between the screens was always in doubt. Moreover in earlier methods the time was not accurately measured. About the end of 1918 the writer proposed to physicists at the Bureau of Standards and to army officers that it was unnecessary for the projectile to make or break electric contacts, that its magnetic effect in passing a station could be registered. He was assured, however, by those with whom he discussed the matter that the experiment had been tried and that the effect could not be obtained. (As a matter of fact this method was being used in England at that time, though the writer did not know this for months afterward.) Procuring apparatus the writer carried through preliminary experiments which showed that the effect could be obtained and registered by means of a string galvanometer.[3] Having made certain of the possibility of registering the magnetic effect the writer then turned his attention to a time source. The ordinary procedure is to use a tuning fork driven by an electromagnet, periodically operated by an electric contact on the fork or on a master fork. A study of this time source showed that the electric contact produced irregularities which had to be eliminated. Then he proposed a method of operating the fork by means of a thermionic vacuum tube. (A method rather similar to this was adopted in England about this time but without the writer's knowledge.) After a little study it was found that the essential feature of this method was to use two electromagnets, one in the grid circuit and one in the plate circuit; the former to be influenced by the motion of a prong of the fork, the latter to drive the fork. Fig. 8 shows a diagram of the wiring as finally modified

[3] The first photographic record for a projectile dropped from a height passing through a solenoid was obtained by the writer by means of an oscillograph.

by Doctor Eckhardt, of the Bureau of Standards, and members of his unit, who have improved the earlier conditions.[4] A tuning fork driven in this way will operate for hours with uniform amplitude, and therefore it can be calibrated with the highest accuracy. Thus time in intervals of 0.001 second can be recorded photographically on a revolving film.

FIG. 8.

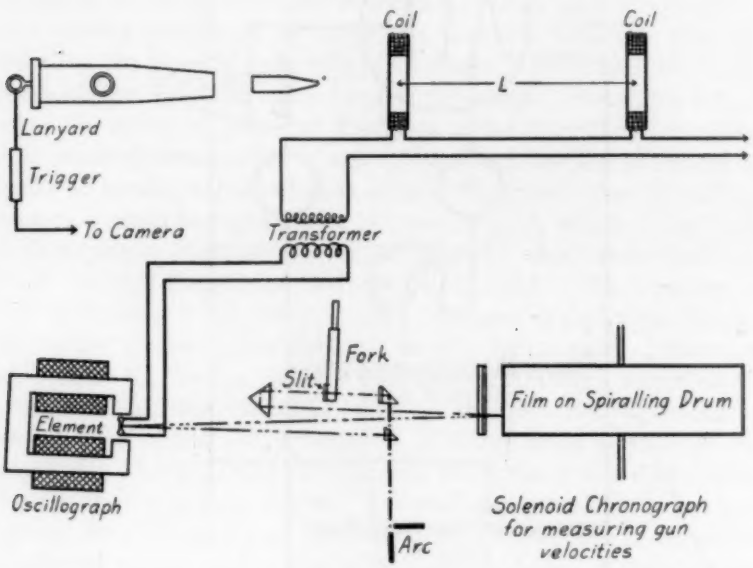


The first magnetic effects due to a shell passing through a solenoid were obtained by the writer at the Aberdeen Proving Ground in August, 1920. They were recorded by means of a string galvanometer. At first it was thought necessary to use a current in a primary coil to magnetize the shell and to use a secondary coil to register the shell's passage. Then it was found sufficient to mag-

[4] I am indebted to Doctor Eckhardt for the diagrams and photographs illustrated in Figures 8, 9, 10, 11, 12.

netize the shell before placing it in the gun. In some cases the change in the earth's flux through the coil due to the passage of a shell (shells from 3 to 6 inches in diameter have been used) was sufficient, and owing to the fact that the effect was easily obtained and that it was difficult to get damped motion in the strings of a galvanometer, an oscillograph was substituted for the former instrument. Fig. 9 shows the wiring arrangements for this experiment. The assembled apparatus as used in the laboratory

FIG. 9.

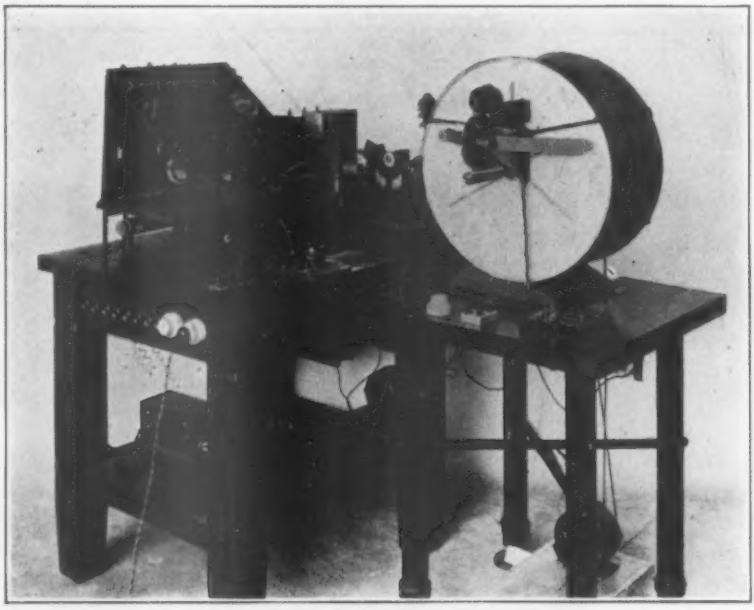


is shown in Fig. 10, and a separate view of the rotating spiralling drum in Fig. 11. In this latter view is seen an ingenious magnetic clutch due to Doctor Karcher, of the Bureau, by means of which the rotating drum can be made to spiral at any given signal. This is an improvement over the mechanical method which the writer used on the earlier drum.

There are a number of interesting mechanical details connected with this apparatus, the evolution of which we have not time here to describe. In the end it may be said that at a given signal, the shutter to the drum can be opened, the gun fired, the revolving drum

can be started spiralling and stopped spiralling at a later signal. The magnetic record of the shell as it passes successive stations is shown by the curved line [5] in Fig. 12. The cross lines are time signals 0.001 second apart. It is clear then that the velocity and therefore the retardation of the shell at numerous points along its path can be measured with high accuracy. As a result of the experiments with

FIG. 10.



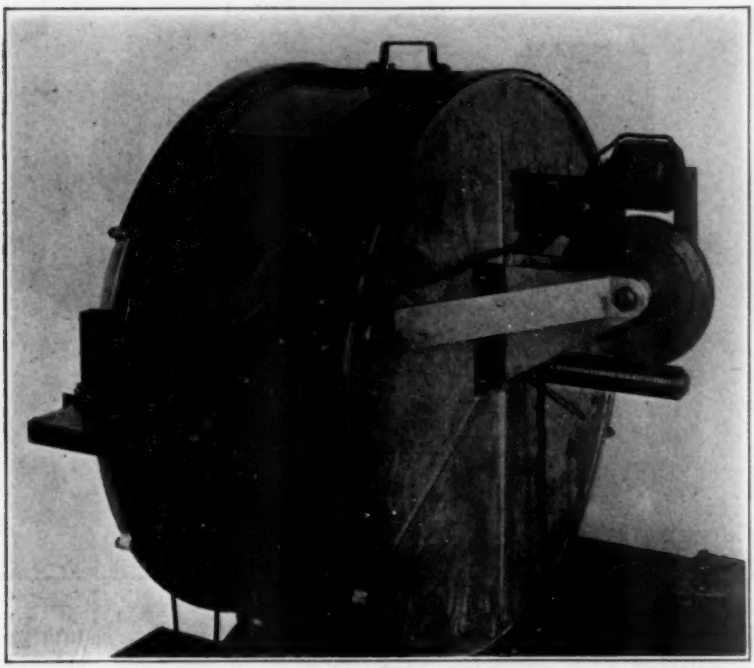
this chronograph it is expected that we will be able to state the retardation of a projectile as a function of its form, velocity, spin, distribution of mass, and the variables denoting the condition of the air.

Turning from problems connected with the projectile to those connected with the gun we find a number claiming our attention. The first is to measure accurately the pressure in a gun and its

[5] If one were certain that the shell passed through the solenoids centrally nose-on to the air one could measure its velocity at each solenoid by the time between the maxima displacements of the curve for that solenoid. This method however would give only a rough approximation to the velocity.

variation with time. Only one gage which can be used in large guns has so far been devised, *viz.*, that due to Doctors Curtis and Duncan, of the Bureau of Standards. It has, so the writer feels, certain defects which ought to be avoided. About two years ago the writer proposed to the technical staff that the piezo-electric effect of quartz be used for this purpose, and he outlined a method which could be followed. He proposed that the charge generated

FIG. 11.



by the pressure be led to the grid-filament circuit of a suitable electron tube or amplifier, and that the resulting variations of the plate current be recorded by means of a sensitive, high-frequency, critically damped oscillograph. It would be necessary of course to connect the metal coating of the quartz plates to the grid so that the latter could be negative and otherwise to ensure high insulation in the circuit. Moreover it would be necessary to use a tube so that a linear relation would obtain between grid voltage, charge and plate

current. For this purpose the writer secured a resistance-coupled ampifier designed to give such a linear relation. This special amplifier, however, would only have to be used where small variations in pressure were desired. The large variations in pressure occurring in a gun could probably be recorded using only a single, rather large triode or pliatron. A pressure gage housing was conducted at Aberdeen, designed to transmit to the quartz plates about one-fiftieth of the total pressure in the gun. But tests at the Bureau on the quartz plates, which were secured by the Ordnance Department after a considerable search, showed that they could withstand the total pressure in the gun. Moreover it was found that there was a linear relation between the charge on the plates and the applied pressure

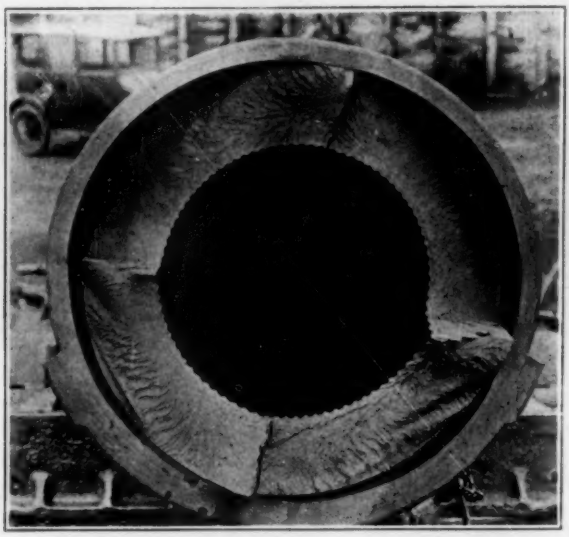
FIG. 12.

up to 50,000 pounds per square inch. These discoveries made possible other methods of recording the pressure than that first proposed. The Bureau has just obtained a time-pressure curve by means of this quartz crystal gage in which the quartz was subjected to the entire pressure in a bomb. The record was made by a very sensitive, long-period galvanometer, the rate of change of deflection of which was proportional to the pressure.

The apparatus just described can also be used to measure the expansion of a gun during firing and its variation with time. For years it has been the custom at proving grounds to " star gage " a gun (*i.e.*, to find its internal diameter) after a certain number of rounds have been fired. This gives the total change in the internal diameter due to erosion and to the expansion of the gun beyond its elastic limit during firing, but it gives no idea of the change of the diameter during the firing. The necessity of such measurements can be seen when we remember that we have no complete explanation of the bursting of certain guns. The popular notion is that the

powder gases experience difficulty in forcing a projectile out of a gun, and that in consequence the pressure rises until it is sufficient to burst the gun by a kind of hydrostatic action. That the action is not static, but often is due to a surging of waves back and forth in the gun, may be seen from a number of photographs of burst gun. Fig. 13 shows the cross section of a gun burst by a small charge after having been previously fired at full charge. The markings show a symmetry which can be accounted for only by a

FIG. 13.



corresponding symmetry of stresses, and this could hardly have been due to a condition during manufacture. It seems more reasonable to suppose that the gun was thrown into vibration by the pressure waves. The effect of these vibrations would be greatest at the muzzle and at other points where there are abrupt changes in mass. That the disruption of the gun generally begins at the muzzle is made evident by many photographs in the possession of the War Department, one of which is shown in Fig. 14. That there are standing waves in the barrel of the gun is strongly suggested by Fig. 13. In any event the measurement of the expansion of the gun during firing and its variation with time is a matter of prime importance.

There have been discussed here a few problems which depend for their solution on the applications of precise experimental methods of modern physics. It is very obvious that the domain embracing warfare of to-day and of the future to which can be

FIG. 14.

applied such methods is very large. However reluctant scientists may be in identifying themselves with war activities they cannot fail to be stimulated by the challenge which these problems throw out.

Microanalytical Methods in Oil Analysis. AUGUSTUS H. GILL and HENRY S. SIMMS, of the Massachusetts Institute of Technology (*Jour. Ind. and Eng. Chem.*, 1921, xiii, 547–552), state that very close analytical results for the saponification number of an oil may be obtained with a sample weighing 15 milligrams, about one one-hundredth the sample usually taken. Likewise very close values for the iodine number may be obtained with a sample of 11 milligrams, approximately one-thirtieth the amount usually used. The specific gravity may be determined on 1 gram of oil. These micro methods are of value when only a small portion of oil is available for analysis, for instance, when oil is extracted from leather, then analyzed.

J. S. H.

Nitrocellulose for the Manufacture of Artificial Leather.—W. K. TUCKER (*Jour. Ind. and Eng. Chem.*, 1921, xiii, 623–624) states that nitrocellulose for use in the manufacture of artificial leather should have a nitrogen content between 11.5 per cent. and 13 per cent., with an average of 12 per cent. Its ash content should not exceed 0.4 per cent. The nitrocellulose is applied to the cloth base as a solution. The chief solvents used are ethyl acetate and acetone oil which is a mixture of methylethylketone with smaller amounts of acetone and related compounds. The solvent is usually diluted with either benzene from coal tar or benzine from petroleum. The amount of actual solvent in a solution rarely exceeds 30 per cent. and may be as low as 10 per cent. The solution is usually prepared so that its viscosity is approximately 40 seconds, *i.e.*, about 40 seconds are required for a steel ball five-sixteenths of an inch in diameter to drop through a column of the solution 10 inches high. The solution preferably should have a temperature of 25° C. during this test. J. S. H.

Thermal Decomposition of Oil Shales.—RALPH H. MCKEE and E. E. LYDER, of Columbia University (*Jour. Ind. and Eng. Chem.*, 1921, xiii, 613–618), have studied the changes which occur during the destructive distillation of oil shales. They find that petroleum oils are not a primary product of the process. The pyrobitumens of the shale first are converted into a heavy solid or semi-solid bitumen; this change takes place at a quite definite temperature, thus it occurred between 400° and 410° C. in the Colorado oil shale used in the experiments. The petroleum oils are then formed by the cracking or cleavage of this heavy bitumen. J. S. H.

Discoloration in Canned Sweet Potatoes.—EDWARD F. KOHMAN, of the Research Laboratory of the National Canners Association (*Jour. Ind. and Eng. Chem.*, 1921, xiii, 634–635), finds that the black discoloration in canned sweet potatoes is due to a reaction between a tannin-like substance in the sweet potatoes and ferric salts derived from the iron of the can. Since access of oxygen is prerequisite for the formation of ferric salts, cans with tight seams are essential in the canning of this product. J. S. H.

Death of Lady Brewster.—Lady Brewster, the second wife of Sir David Brewster, died recently at the age of ninety-four. She was married to Sir David in 1857, when he was seventy-six and she about thirty. A daughter was born to them a few years later. Brewster died in 1868. He was one of the most distinguished physicists of his time, his work being especially in optics. He constructed the first lens stereoscope and perfected the kaleidoscope. He also made many important researches in polarized light and devised important improvements in the British lighthouse system.

H. L.

A MECHANICAL FREQUENCY-METER OF TELEPHONIC RANGE.*

BY

A. E. KENNELLY, Sc.D.

Member of the Institute.

and CH. MANNEBACK.

IN the JOURNAL OF THE FRANKLIN INSTITUTE for May, 1919, Dr. L. V. King described and illustrated a form of mechanical frequency-meter which he called a "sonometer." His illustration of the device is reproduced in Fig. 1, ACB is a fine steel wire fastened at one end to the top of the cathetometer and soldered at the other end to a known weight W + W. This wire is supported in front of a telephone electromagnet T energized by the alternating current whose frequency is to be determined. The position of the bevel edge C is adjusted until, with the electromagnetic T excited, the wire BC is set into resonant vibration. The frequency of the exciting current is then

$$f = \frac{1}{2l}\sqrt{\frac{T}{m}} \quad \frac{\text{cycles}}{\text{sec}} \qquad (1)$$

where T is the total tension on the wire in dynes.
m– the linear mass of the uniform wire in gm/cm.
l– length of the vibrating segment BC in cm.

DESCRIPTION OF THE FREQUENCY-METER.

The laboratory frequency-meter here to be described is a development of the King sonometer, undertaken in the electrical engineering laboratories of the Massachusetts Institute of Technology.

In the King sonometer described, the linear mass was 1.373 gm/cm., and the available range of frequency was 360 to 1600 cycles per second.

The final form of the instrument is shown in Fig. 2. The wire used was a silver-plated steel wire, known as a "black diamond" string or "guitar B" as furnished by the National Musical Co., of New Brunswick, New Jersey, having a diameter of 0.350 mm

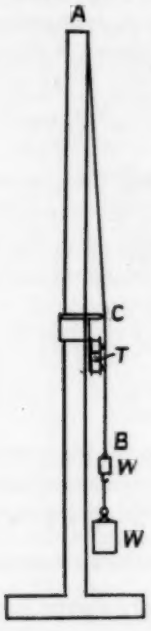
* Communicated by Dr. Kennelly.

which was entirely uniform over the length used, so far as ordinary micrometer measurements showed. Its linear mass was 7.585 gm/cm., as a mean of three determinations in close agreement.

The length of wire in the vibrating segment can be varied between 3 cm. and 60 cm., over a working range of from 250 to 2500 cycles per second.

A rectangular brass bar CD, Fig. 2, 60 cm. high, 2.5 cm. wide and .83 cm. thick, was graduated to a millimeter scale of length.

FIG. 1.

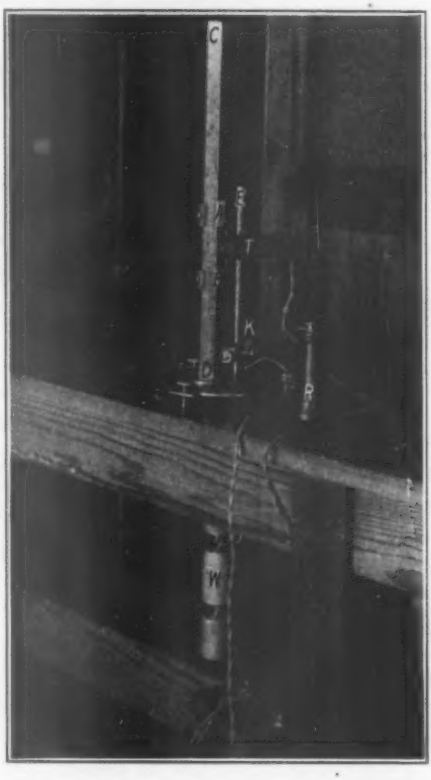


This vertical bar is supported on a flat circular brass base plate, 6.4 mm. thick and 250 mm. in diameter. This plate is, in turn, supported by three ordinary brass leveling screws, passing through projections at the edge of the plate. The bar is soldered to a small brass disk, 6.4 mm. thick and 55 mm. in diameter, which is fastened by screws to the base plate. The base plate also supports the vertical brass rod, which serves to carry the operating telephone T, mounted in a wooden frame at an adjustable height approximately half way between the bevel edges A and B. In practice, the height

of this telephone T does not have to be varied, and, in any future similar instrument, might be left fixed.

A rack and pinion screw, on the frame of the telephone T, enables the horizontal distance between the telephone poles and the vertical wire to be conveniently adjusted. The cover and diaphragm

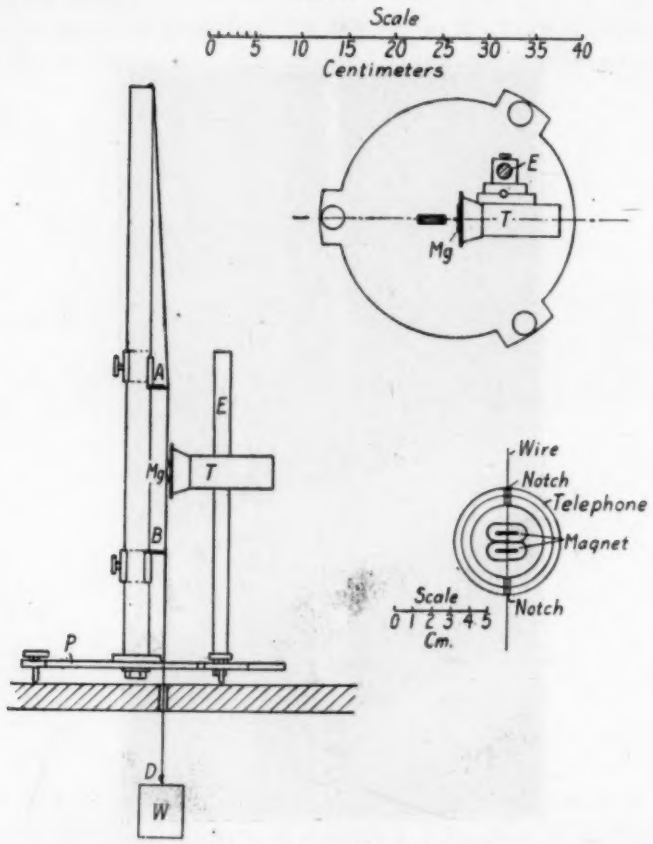
FIG. 2.



of this telephone are removed. The metallic lip has a notch in it above and below, to permit the poles to approach the wire closely. The magnet of the telephone might be removed from its frame, as in the King sonometer, but the usual composition case affords a convenient mechanical protection. The poles should occupy the horizontal positions shown in Fig. 3, and no advantage has been found to attend any other position of these poles with respect to

the wire. The vertical steel wire tends to close the magnetic circuit of the telephone in the same manner as though it were a central strip cut from the ordinary diaphragm. At low frequencies, with long vibrating wire segments, the distance from poles to wire may

FIG. 3.



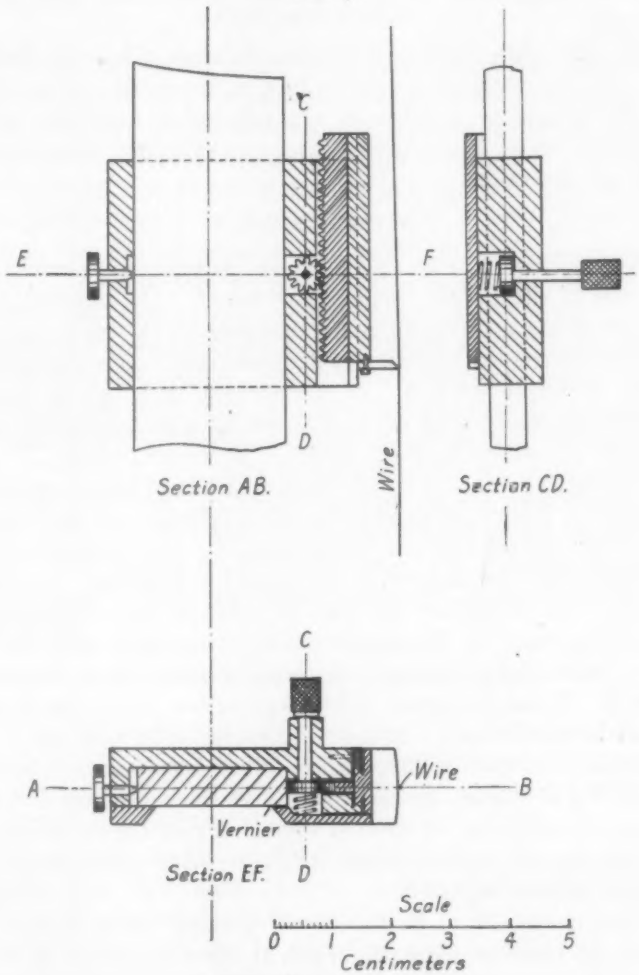
Mechanical measurement of the frequencies.

be 2.5 mm., to allow of free vibration; but at high frequencies, with short vibrating segments, the distance may be reduced to 0.5 mm., or less.

The lower brass bevel edge B has a simple sliding motion on the vertical bar, with a clamp screw to hold it in place. The upper brass edge A has, in addition to a clamp screw, a rack and pinion

vernier adjustment, as shown in Fig. 4. Final adjustments of the length to the vibrating wire segment are effected easily to a tenth

FIG. 4.



Details of the upper bevel edge.

millimeter at this vernier pinion screw. For convenience of reading the millimeter scale, the vertical rod has an arbitrary zero at a point about 20 cm. above the base. From this zero, the scale numbers

increase in each direction, so that the total length of the vibrating wire segment is always the numerical sum of the two bevel-edge readings. If this arrangement were not employed, the reading of the lower edge would have to be subtracted from that of the upper edge.

The steel wire is allowed to pass through a hole in the base plate, and also through a similar hole in the table. Beneath the table, the weight or weights W are suspended from the wire in the ordinary way. Fig. 3 gives drawings of the instrument in plan, and in side elevation.

METHOD OF USE.

The telephone T, Figs. 2 and 3, has its terminals connected to the source of alternating current to be tested. The particular telephone used has a direct-current resistance of 86 ohms and is a Western-Electric instrument of a standard type. At a frequency of 2000 ω, an impressed e.m.f. of 5 volts rms. at telephone receiver terminals, produced a current of about 8.75 milliamperes, representing an impedance in the receiver of about 570 ohms.

In order to form an idea of the magnitude of the frequency to be measured, it is convenient to lay the diaphragm on the receiver, so as to hear the pitch of the sound emitted. This preliminary test also shows that the receiver is in operative condition. A rough estimate of the frequency can thus be made by ear. Reference to a calibration chart for the instrument will then show what distance between bevel edges AB may be looked for with a given suspended weight W. If the frequency is low, say below 500 ω, the distance AB will be fairly large (at least 100 mm. with 900 gm.), and mechanical resonance is then easily detected as soon as the right distance AB is found, by the bowing of the wire into a ventral segment, as well as by its emitted sound. From the measured distance AB, and the applied weight W, the frequency can then be read from the calibration chart.

If the frequency to be measured is much above 500 ω, the distance AB must be reduced. Thus at 2500 ω, the distance AB becomes 52.5 mm. with a total weight of 5.4 kg. In such a case, the poles of the receiver T may have to be brought close up to the wire and the amplitude of resonant vibration is small. In a quiet room, the point of resonance can still be detected by sound, but the amplitude of vibration may escape notice, depending on the exci-

tation of the receiver. In a noisy room it may be necessary to use an auxiliary indicating device, and it is often convenient to use the latter, even under favorable conditions of measurement.

AUXILIARY DEVICE.

Various devices for indicating mechanical resonance have been tried. A very satisfactory device is found in a carbon microphone button, brought into contact with the wire below the bevel edge B, and therefore outside the principal vibrating segment. A unidirectional current of a few milliamperes is passed through the microphone button and a head telephone from a local 4-volt storage battery. In Fig. 2, however, an auxiliary hand telephone R is shown. The auxiliary receiver R reproduces in the ear of the observer, the tone of the exciting telephone receiver T. When mechanical resonance is attained, by securing the proper distance AB between bevel edges, the auxiliary telephone gives a clearly distinguishable loud tone; so that it is only necessary to separate the edges A and B slowly, starting both from their zero or coincidence position, to recognize resonance in the auxiliary receiver R. The microphone button is supported on the end of a flexible spiral spring, which, in turn, rests in a wooden block on the upright brass rod E. The contact between the button and the wire is normally so light that no appreciable change occurs therefrom in the tension of the wire.

CALIBRATION OF THE FREQUENCY-METER.

The instrument was calibrated in two ways, namely, (1) by measuring the rotary speed of the alternator supplying the telephonic frequency, (2) by measuring the frequency delivered to the telephone T with an electric bridge.

Fig. 5 shows an electrically driven stroboscopic fork,[1] mounted in front of a disk target carried by the rotor of the alternator diverting the frequency to be measured. The fork F can be adjusted in frequency, and in vibration, over a range of about 10 per cent., with the aid of the pulleys P and sliding weights S. This fork, in turn, had its calibration checked with the aid of speed

[1] "The Measurement of Rotary Speeds of Dynamo Machines by the Stroboscopic Fork" by A. E. Kennelly and S. Whiting, *Proc. A.I.E.E.*, June, 1908, p. 631.

counters. Fig. 6 is a calibration diagram, drawn on logarithm paper, with the frequency as ordinates, against the distance between bevel edges as abscissas, for five different values of the suspended weight. These give five parallel straight lines computed according to the formula

$$f = \frac{1798}{l}\sqrt{W} \quad \text{cycles/sec.}$$

where l is the bevel-edge distance expressed in millimeters, and W is the suspended weight in grams. The actual calibration agreed substantially with these straight-line graphs, although the observations deviated occasionally to one side or the other. Higher frequencies and shorter bevel-edge distances call for heavier weights, while, on the contrary, the smaller weights should be used with the lower frequencies. Consequently, assuming that the proper weight is used, the frequency is read off the corresponding straight-line graph in Fig. 5.

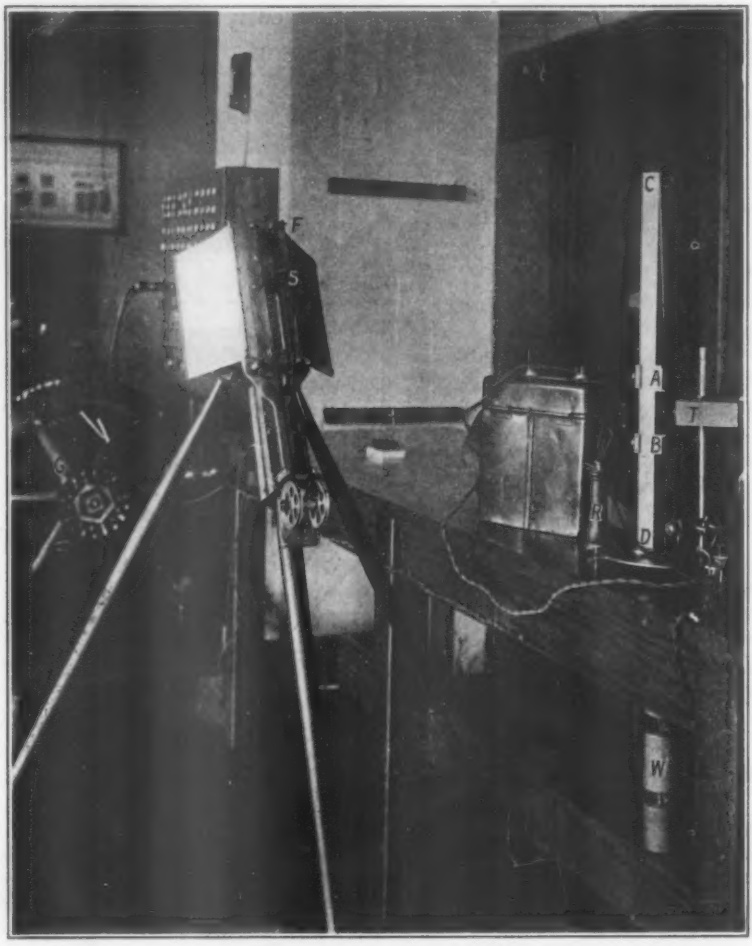
PRECISION.

The instrument is so sensitive to frequency, as was pointed out by Doctor King, that a bevel-edge setting can be adjusted and read ordinarily to one-tenth of a millimeter. If the bevel-edge distance was say, 10 cm., this would correspond to a nominal precision of 1 per mil. of the measured frequency. Owing, however, to certain errors in the instrument, the actual precision attainable has been distinctly less, and under favorable weight conditions, the actual deviation of a single observation was sometimes 1.5 per cent., or an error 15 times larger than that estimated by adjustment of length. It would appear that the error of observation, as that term is ordinarily understood, is small and rarely exceeds 2 or 3 per mil.; whereas systematic errors inherent in the construction used, attain 10 or 15 per mil. (1 to 1.5 per cent.), assuming that favorable weights are retained. If unsuitable weights are employed, the errors may be still greater.

It is supposed that the systematic errors here referred to are due to deviations of the actual nodal point from the bevel edges. In other words, if the vibrating wire formed its nodal points exactly at the bevel edges, these systematic errors might vanish. Further experimental investigation might advantageously be

directed toward eliminating these systematic errors, so as to increase the available precision.

FIG. 5.



ADVANTAGES OF THE INSTRUMENT.

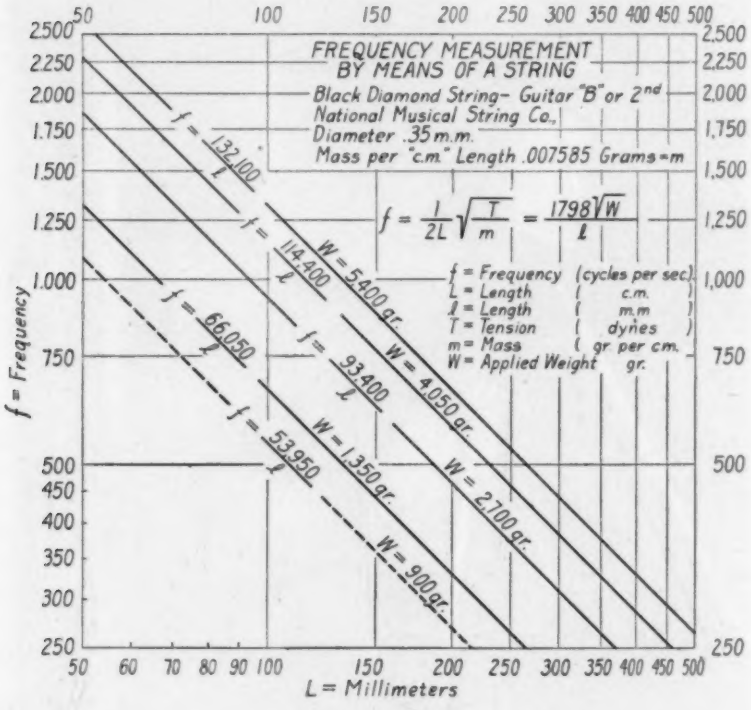
(1) The instrument is easily constructed. Although the form described is not very portable, it is well adapted to telephone laboratory use, and is not likely to get out of order.

(2) The instrument enables a frequency measurement to be made very swiftly.

(3) Very small power is consumed.

(4) It has been shown experimentally that the measurements are but little affected by wave shape of the current whose frequency is determined.

FIG. 6.



(5) Large available frequency range. The instrument here described covers a tenfold range.

DISADVANTAGES.

(1) Owing to the numerous possible nodes of vibration of a monochord, care must be exercised to measure the fundamental frequency, and not to be misled by mechanical overtones.

(2) Proper weights should be used with the instrument, in

accordance with the frequency to be measured. In the instrument described the recommended weights are as follows:

Frequency Cycles per second	Best weight Grammes
250 to 500	900
500 to 750	1350
750 to 1250	2700
1250 to 1750	4050

In the calibration of any particular instrument of this type, the best weights to use can readily be determined experimentally.

The authors desire to acknowledge their indebtedness, not only to Prof. L. V. King for the original idea of the instrument, but also to Prof. V. Bush, and to Prof. F. S. Dellenbaugh of our department, for help and valuable suggestions.

Death of Lippmann.—J. F. Gabriel Lippmann, the inventor of the Lippmann process of color photography, died in July last. He was born in 1845, in Luxembourg, of Alsatian-Lorraine parents, but went early to France, where he was educated and where his scientific work was all carried out. He received many honors, the greatest being the award of a Nobel prize in physics in 1918. His first scientific contribution was an essay on electro-capillarity, submitted as a thesis for the degree of Doctor of Sciences. In 1886 he was elected a member of the Academy of Sciences and numerous other appointments of trust and research were later given to him. The first notice of his remarkable procedure for obtaining color photographs by the interference method was made to the Academy in February, 1891, when he exhibited a photograph of a spectrum in its true colors. The paper appears on *C.r.*, 1891, v. 112, 274. In 1892 the French Photographic Society awarded him the Jannsen medal, and in 1897, the "Progress" medal was bestowed by the Royal Society of London. Lippmann's interest in photography continued through life and among his later contributions is one on the use of color screens in orthochromatic work.

The Lippmann process is simple in general principle but has never come into active use. It was not patented, and the inventor is said to have expressed regret that he did not at once take out patents, as he thought possibly this might have encouraged commercial exploitation. A recent issue of *La Revue Française de Photographie* gives the story of his life in some detail, and a brief account of the particular procedure upon which his fame principally rests.

H. L.

Dry Method for the Preparation of Lead Arsenate. O. W. BROWN, C. R. VORIS, and C. O. HENKE, of Indiana University (*Jour. Ind. and Eng. Chem.*, 1921, xiii, 531–533), have devised a

method for the preparation of lead arsenate in the dry way. Lead monoxide (litharge) and arsenious oxide (white arsenic) are mixed in the proportion of 3 molecules of the former to 1 molecule of the latter. The mixture is roasted in a rotatory furnace; the optimum temperature for roasting is about 450° C. The two oxides combine to form lead arsenite, and this compound is oxidized to triplumbic arsenate $Pb_3 (AsO_4)_2$. The oxidation at first is very rapid, then occurs very slowly. The amount of water-soluble arsenic in the product depends on the time and the temperature of roasting and on the relative proportions of the two oxides in the initial mixture. The process may be so regulated that the amount of water-soluble arsenic in the product is much less (approximately one-third) than that present in lead arsenate manufactured by the wet process.

J. S. H.

Influence of the Moisture Content Upon the Deterioration of Raw-dried Vegetables During Common Storage. H. C. GORE and C. E. MANGELS (*Jour. Ind. and Eng. Chem.*, 1921, xiii, 523–524) have studied the influence of the moisture content of raw-dried or dehydrated vegetables upon their deterioration during the storage in air-tight containers at ordinary temperatures. The moisture content of the dehydrated product must not exceed the following values on entering storage if the distinctive color and flavor are to be well retained for 6 or more months: Carrots 4.99 to 7.39 per cent., turnips 5.00 per cent., onions 5.74 to 6.64 per cent., spinach 3.81 to 5.38 per cent., cabbage 3.00 to 3.34 per cent.

J. S. H.

Natural Indigo.—The culture of the indigo plant and the manufacture of indigo from it are described by W. R. G. Atkins (*Science Progress*, 1921, xvi, 56–70). The plants are cut and steeped in water at a temperature of at least 90° F.; a bacterial fermentation occurs, and the indican of the plant tissues passes into solution, and is converted into indoxyol. The resulting aqueous extract is beaten; the indoxyol is oxidizd to indigotin or indigo blue, which usually precipitates. If the supernatant liquid has a green color as the result of abnormal fermentation, the indigo in it is precipitated by addition of a small amount of dhak gum, and the yield is thereby increased. The indigo passes through processes of boiling, filtering, and drying. Natural indigo usually occurs in commerce as a cake with an indigotin content ranging from less than 50 per cent. to 70 per cent.; it is also being placed on the market as a paste. Synthetic indigo may ultimately displace the natural product as an article of commerce, However, the present high prices of raw materials, coal, and labor make the price of synthetic indigo so high that the production of natural indigo promises to be profitable for many years to come.

J. S. H.

PRESENTATION OF THE FRANKLIN MEDAL, AND CERTIFICATES OF HONORARY MEMBERSHIP, MAY 18, 1921.

At the Stated Meeting of the Committee on Science and the Arts, held January 5, 1921, the following resolutions were adopted:

"*Resolved*, That The Franklin Medal be awarded to Professor Charles Fabry, of the University of Marseilles, France, in recognition of his numerous and highly important contributions in the field of physical science, particularly the solution of optical and spectroscopical problems of fundamental importance."

"*Resolved*, That The Franklin Medal be awarded to Frank J. Sprague, of New York, New York, in recognition of his many and fundamentally important inventions and achievements in the field of electrical engineering, notably his contributions to the development of the electric motor and its application to industrial purposes, and in the art of electric traction, signally important in forming the basis of world-wide industries and promoting human welfare."

CORRESPONDENCE WITH MEDALISTS.

THE FRANKLIN INSTITUTE
OF THE STATE OF PENNSYLVANIA
Philadelphia

OFFICE OF THE SECRETARY — JANUARY 11, 1921.

Professor Charles Fabry,
1 rue Clapier,
Marseilles, France.

SIR:

I have the honour to inform you that The Franklin Institute has awarded you The Franklin Medal, founded for the recognition of those workers in physical science and technology, without regard to country, whose efforts, in the opinion of the Institute, have done most to advance a knowledge of physical science or its applications. The award is minuted as follows:

> "That The Franklin Medal be awarded to Professor Charles Fabry, of the University of Marseilles, France, in recognition of his numerous and highly important contributions in the field of physical science, particularly the solution of optical and spectroscopical problems of fundamental importance."

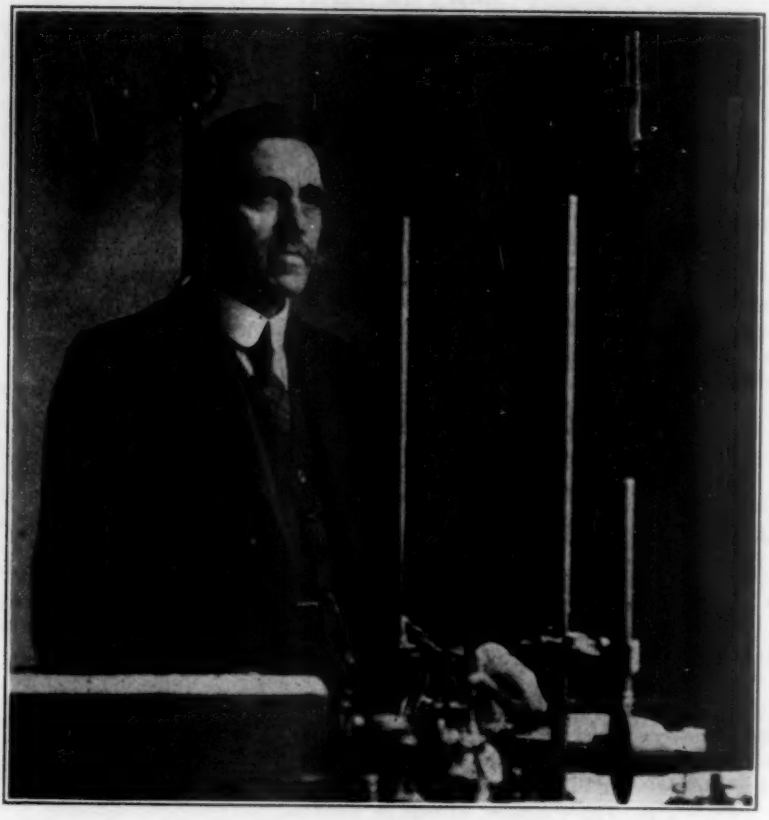
The medal and accompanying certificate are being prepared, and Mr. J. J. Jusserand, your Government's Ambassador at Washington, is being requested to come to the Institute on the afternoon of Wednesday, May 18th, to receive this medal and certificate on behalf of his Government for you.

I am,

Respectfully,

(Signed) R. B. OWENS,
Secretary.

RBO:H



Ch Fabry

THE FRANKLIN INSTITUTE
OF THE STATE OF PENNSYLVANIA
Philadelphia

OFFICE OF THE SECRETARY

JANUARY 11, 1921.

Mr. Frank J. Sprague,
165 Broadway, New York City.

SIR:

I have the honour to inform you that The Franklin Institute has awarded you The Franklin Medal, founded for the recognition of those workers in physical science and technology, without regard to country, whose efforts in the opinion of the Institute have done most to advance a knowledge of physical science or its applications. The award is minuted as follows:

"That The Franklin Medal be awarded to Frank J. Sprague, of New York, New York, in recognition of his many and fundamentally important inventions and achievements in the field of electrical engineering, notably his contributions to the development of the electric motor and its application to industrial purposes, and in the art of electric traction, signally important in forming the basis of world-wide industries and promoting human welfare."

The medal and accompanying certificate are being prepared, and I am requested, on behalf of our management, to extend to you a cordial invitation to come to the Institute on the afternoon of Wednesday, May 18th, to receive this medal and certificate from our President, Dr. Walton Clark.

I am,

Respectfully,

(Signed) R. B. OWENS,
Secretary.

RBO:H

THE FRANKLIN INSTITUTE
OF THE STATE OF PENNSYLVANIA
Philadelphia

OFFICE OF THE SECRETARY

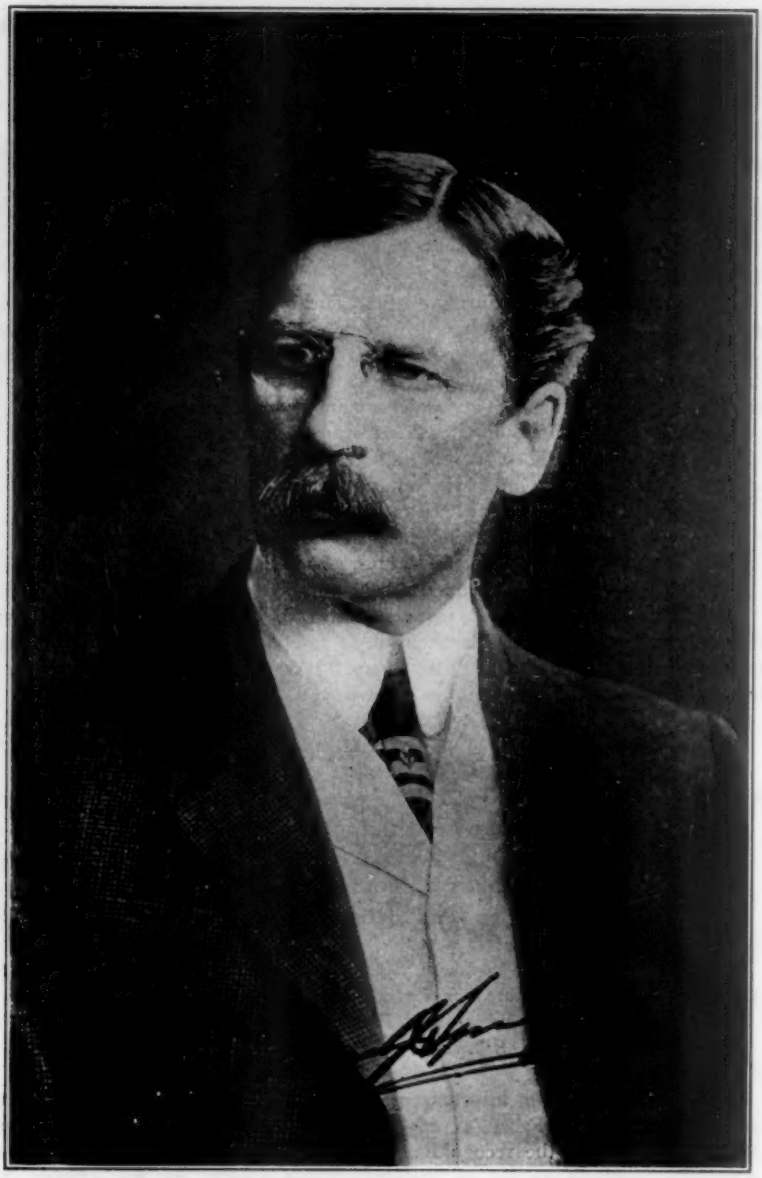
JANUARY 11, 1921.

M. J. J. Jusserand, Ambassador Extraordinary
and Plenipotentiary, Republic of France,
French Embassy, Washington, D.C.

SIR:

I have the honour to inform you that The Franklin Institute has awarded to Professor Charles Fabry, 1 rue Clapier, Marseilles, France, The Franklin Medal, founded for the recognition of those workers in physical science or technology, without regard to country, whose efforts, in the opinion of the Institute, have done most to advance a knowledge of physical science or its applications. The award is minuted as follows:

"That The Franklin Medal be awarded to Professor Charles Fabry, of the University of Marseilles, France, in recognition of his numerous and highly important contributions in the field of physical



Theodore Roosevelt

science, particularly the solution of optical and spectroscopical problems of fundamental importance."

The medal and accompanying certificate are being prepared, and I am requested, on behalf of our management, to extend to you a cordial invitation to come to the Institute on Wednesday, May 18th, to receive this medal and certificate from our President for Professor Fabry.

Two Franklin Medals have been awarded this year, one to Professor Fabry and one to Mr. Frank J. Sprague, Engineer and Inventor, of New York City, U.S.A.

An invitation will be extended to you and to Mr. Sprague to be the guests of honour at a dinner following the presentation ceremonies. This invitation will be formally sent you at a later date by the President of the Institute, Dr. Walton Clark.

I am,

Your Excellency's humble servant,

(Signed) R. B. OWENS,
Secretary.

RBO:H

FRANK J. SPRAGUE
165 Broadway
New York

JANUARY 18, 1921.

DR. R. B. OWENS, *Secretary,*
The Franklin Institute,
Philadelphia, Pa.

DEAR DR. OWENS:

I beg to acknowledge your letter of Jan. 11th, conveying official information that The Franklin Institute has awarded me "The Franklin Medal," and stating the grounds on which the award has been made, as well as inviting me to be present to receive it on the 18th of May.

I shall be glad to accept this invitation and to then formally express, as I do now also, my appreciation of the gratifying recognition of my work by the bestowing of this great honor.

I am deeply sensible of the fact that there are many who are in the highest degree deserving of recognition by the Institute and that in thus being singled out for distinction I am specially indebted to those on whom the responsibility falls for selection.

Pray accept for yourself my personal thanks, and likewise my deep sympathy and concern over the report that you are temporarily prevented from active participation in your duties as Secretary, with the hope of your early complete recovery.

Sincerely yours,

(Signed) FRANK J. SPRAGUE.

AMBASSADE
DE LA RÉPUBLIQUE FRANÇAISE
AUX ETATS-UNIS

WASHINGTON, le January 24, 1921.

DEAR SIR:

I beg to acknowledge receipt of your letter of the 11th, and to express the feeling of gratification with which I learnt of the bestowal of The Franklin Medal on my compatriot, Professor Charles Fabry.

I cannot, owing to my duties here, pledge myself in advance to go to Philadelphia on May 18th, to receive in person the medal and the accompanying certificate. In case I am prevented, I would appoint, if agreeable to you, a representative to act in my stead.

Believe me, with best regards,

Sincerely yours,

(Signed) JUSSERAND.

MR. R. B. OWENS, *Secretary,*
The Franklin Institute,
Philadelphia, Pa.

FACULTÉ DES SCIENCES
PARIS
PHYSIQUE
LABORATOIRE D'ENSEIGNEMENT
1, rue Victor-Cousin (5e Arr.).

PARIS, le 30 Janvier, 1921.

MY DEAR DR. OWENS:

I received yesterday your letter, coming via Marseille, informing me of the award to me of The Franklin Medal. I appreciate deeply the honour done me by The Franklin Institute. The list of medalists includes the names of scientists of world-wide prominence, and I am very proud to be placed among them by the Institute. Moreover, the medal will be for me a recollection of a visit to America in war time, and will recall to me the gratefulness that every Frenchman owes to your country.

Two weeks ago I was appointed a professor at the University of Paris (Sorbonne), and am already at work in my new laboratory.

It is not without sorrow that I left my laboratory at Marseille, in which I worked for so long a time, but my colleagues in Paris insisted so strongly upon my coming to them that I felt it necessary to accept.

I hope my new position will give more opportunity to meet my American friends.

Very sincerely yours,

(Signed) CHARLES FABRY.

UNIVERSITÉ DE PARIS
FACULTÉ DES SCIENCES
PHYSIQUE
LABORATOIRE D'ENSEIGNEMENT
1, rue Victor-Cousin (5e arr.).

PARIS, 10 APRIL, 1921.

CHER DR. OWENS:

C'est un grand honneur pour moi d'être placé sur la liste des members honorare de l'Institut Franklin. Veuillez exprimer mes cordiaux remerciements au President et aux members de l'Institut, sans oublier vous-mème.

J'éspere que vous avez reçu le manuscript de mon "paper," que je vous ai envoyé de Marseille il y a environ deux semaines. Je regrette beaucoup de ne pouvoir le lire moi-mème à votre réunion de mai; mais la lecture sera beaucoup plus agréable si elle est fait par mon ami Ames. Veuillez le remercier pour la peine qu'il se donne à cette occasion.

Nous avous le plaisir, en ce moment d'avoir à Paris votre illustre compatriote Michelson, qu'est venu dans notre Universite comme "exchange Professor." Les leçons sont un grande succès.

Je suis particulièrement heureux des bonnes nouvelles que vous me donnez de notre vue. La nécessité de porter des verres n'est nullement génante; voilà longtemps que je ne peux pas lire autrement et que j'ai pris l'habitude de considerer mes verres comme faisant partie de ma personne, comme mes bras ou mes jambes.

Le text de la "minute" est très bien, excepte qu'il est beaucoup trop èlogieux pour mes travaux.

Cordialement à vous,
CH. FABRY.

CONFERRING OF HONORARY MEMBERSHIP.

AT the Stated Meeting of The Franklin Institute, held March 16, 1921, the following resolution was adopted:

> "*Resolved*, That General John J. Pershing be elected an Honorary Member of The Franklin Institute, in recogniton of his signal success in the use of scientific and technical methods and means in modern warfare."

CORRESPONDENCE.

MARCH 17, 1921.

General John J. Pershing,
General of the Armies of the United States,
War Department, Washington, D. C.

SIR:

I have the honour to advise you that at a regular meeting of the Institute, held on the evening of Wednesday, March 16th, the following resolution was unanimously adopted:

"*Resolved*, That General John J. Pershing be elected an Honorary Member of The Franklin Institute, in recognition of his signal success in the use of scientific and technical methods and means in modern warfare."

It is hoped that it may be possible and agreeable to you to be at our May meeting to be held on the afternoon of Wednesday, May 18th, and receive from our President an engrossed copy of the Institute's action.

I am,

Your very obedient servant,

(Signed) R. B. OWENS,
Secretary.

RBO:H

GENERAL OF THE ARMIES
Washington

MARCH 21, 1921.

Dr. R. B. Owens, Secretary,
The Franklin Institute,
Philadelphia, Pennsylvania.

MY DEAR DOCTOR OWENS:

I am deeply gratified at the action of The Franklin Institute on March 16th in electing me to honorary membership, and I am very proud to accept this mark of distinction at their hands. I hope you will convey to the officials of the Institute my deep appreciation of this honor and my hearty thanks for their courtesy.

It is not possible for me to say at this time whether I can attend the meeting on the afternoon of May 18th, but if my plans will permit, I shall be very happy to do so.

With assurances of my high regard,

Very Sincerely yours,

(Signed) JOHN J. PERSHING.

PROGRAMME OF MEETING, MAY 18, 1921.

Presentation of Certificate of Honorary Membership to General John J. Pershing, General of the Armies of the United States.

Presentation of the Franklin Medal and Certificate of Honorary Membership to His Excellency, M. Jusserand, French Ambassador, on behalf of his Government, for Professor Charles Fabry, the University of Paris, Paris, France.

Presentation of The Franklin Medal and Certificate of Honorary Membership to Frank J. Sprague, New York City.

Papers:

"Studies in the Field of Light Radiation." By Charles Fabry, D.Sc. Read by Joseph S. Ames, Ph.D., LL.D., The Johns Hopkins University, Baltimore, Maryland.

"Electric Traction—A Review." By Frank J. Sprague.

PRESENTATION OF THE FRANKLIN MEDAL AND CERTIFICATE OF HONORARY MEMBERSHIP TO PROFESSOR CHARLES FABRY AND MR. FRANK J. SPRAGUE, AND CERTIFICATE OF HONORARY MEMBERSHIP TO GENERAL JOHN J. PERSHING.

IN calling the meeting to order Mr. Coleman Sellers, Jr., Vice-President of the Institute, said:

I greatly regret that severe illness will prevent our President, Dr. Walton Clark, from being with us this afternoon. Doctor Clark has taken the greatest interest in this meeting and I am sure that his inability to be present is as great a disappointment to him as it is to us.

This is a regular stated monthly meeting of the Institute, held at an unusual hour, for the greater convenience of some of our distinguished guests.

As this meeting is dedicated to a particular purpose, the usual order of business will be dispensed with.

The special business of this meeting will be the presentation of the Franklin Medals which have been awarded to Dr. Charles Fabry, of Paris, and to Mr. Frank J. Sprague, of New York City.

A further object of this meeting will be to formally testify our appreciation of the character, attainments and services of John J. Pershing, General of the Armies of the United States, recently appointed by the President as Chief of the General Staff of the Army.

The Chairman then recognized Dr. Harry F. Keller, who introduced General Pershing as follows:

Mr. President: At a stated meeting of the Institute held on the evening of Wednesday, March 16, 1921, the following resolution was unanimously adopted:

> *Resolved,* That General John J. Pershing, General of the Armies of the United States, be elected to Honorary Membership in the Institute, in recognition of his signal success in the use of scientific and technical methods and means in modern warfare.

I have the honour to present to you General Pershing.

The Chairman in presenting the Certificate of Honorary Membership said:

General Pershing: I have the honour in the name of The Franklin Institute, and upon the unanimous recommendation of

its Committee on Membership, the unanimous endorsement of its Board of Managers, and the unanimous vote of its stated meeting, to present to you a Certificate of Honorary Membership in The Franklin Institute of the State of Pennsylvania. The members of the Institute, remembering your brilliant services to our country and to mankind rendered on the fields of heroic France, realize that you honour the Institute in accepting this Certificate.

General Pershing, in accepting the Certificate of Honorary Membership, said:

Mr. Chairman, Ladies and Gentlemen: It would be very difficult for me to express clearly on this occasion my very deep appreciation of the honour that has been conferred upon me by this world-renowned Institute. Recognized, as it is, in the scientific world as a leader, I am doubly proud to become one of its members. All other things being equal, we know that which leads in science would naturally lead in war. The contributions made to the conduct of the war by scientific and technical men of America cannot be overestimated. The contributions made by the distinguished members of The Franklin Institute deserve especial consideration and praise at the hands of those who are in a position to know what their services were and the American people in general know that The Franklin Institute especially deserves credit for the number of its members who saw active service abroad, one in twelve of the membership of The Franklin Institute having served in the World War; eighty-four held commissions in the army, and I am glad to observe that fifty-five of them held positions in the army with the rank of field officer or higher, twenty-eight others were officers in the Navy, and fifty-nine were on active government service without military rank. More than twenty members were honoured for their service by the French, British, Belgian, Italian or American Government.

It is an especial pleasure to me, and one which I wish to state here to this gathering, to meet a very old-time friend of mine, your Secretary, Doctor Owens, and to have him to receive me on this occasion, bringing back as it does our service together some twenty-five years ago when we were both young men at the University of Nebraska.

I again express my very deep appreciation of the honour

you have conferred upon me by making me an Honorary Member of this illustrious Institute.

Dr. Joseph S. Ames, of the Johns Hopkins University, was then recognized and presented the following statement of the work of Professor Charles Fabry:

Mr. President: Your Committee on Science and the Arts in its recommendations for the award of the Franklin Medals has, in my judgment, shown rare wisdom and discrimination. The essential condition in the deed of gift is that the medals shall be bestowed upon those "whose efforts, in the opinion of the Institute, have done most to advance physical science or its applications." The first of these medals to be awarded to-day cannot, unfortunately, be placed in the hands of the recipient himself, as his duties in Paris make it impossible for him to be with us. But we are honoured by the presence of the official representative of the French Republic, who has graciously consented to accept, on behalf of his Government, The Franklin Medal for his fellow-countryman, Dr. Charles Fabry, Professor of Physics in the University of Paris. It is awarded him in recognition of his invaluable contributions to the science of spectroscopy.

Charles Fabry was born at Marseilles on June 11, 1867; and, after preliminary studies in his native city was admitted to the famous École Polytechnique in 1885, where he remained for three years. He then returned to Marseilles. In 1892, while a professor of physics at the Lycée of Bordeaux, he presented his thesis for the degree of Doctor of Physical Sciences to the Faculty of the Sciences of the Academy of Paris, and received his degree. Two years later he was appointed Professor at the University of Marseilles and during the present year has been called to the University of Paris.

Fabry's thesis bore the title—expressed in English—"Theory of the Visibility and Orientation of Interference Fringes" and was dedicated to Professor Macé de Lépinar, of the University of Marseilles. To this renowned physicist we owe a great debt for attracting the attention of his pupil to the important field of spectroscopy. The thesis is the extension of work previously done by Fabry and reported briefly in the Comptes Rendus of the Academy two years before. It deals with the most interesting subject of the localization in space of interference fringes, which

are seen in so many optical phenomena, and with which every one is familiar from the brilliant colors of soap-bubbles, of thin layers of oil or gasoline and of the opal. The memoir shows a happy combination of mathematical knowledge, of physical insight and of experimental ability; a combination most rare. It was the first of a long series of papers, all connected with spectroscopy, which have continued to the present time and which have delighted and inspired every student of Physics. His theoretical papers have never given us theories unsupported by experiment, but have always led us farther into actual knowledge of the constitution of matter; and his experimental investigations have been models of clearness of purpose and of technical ability.

The name Fabry is known to every elementary student of physics, even in his college days, from association with a new form of optical apparatus which bears the name Fabry-Perot interferometer, and which has contributed more than any other instrument to our knowledge of spectra. The results of modern spectroscopy rest entirely on its use. It was devised, in collaboration with M. Perot, in 1897, and furnishes us the means of measuring the wave-lengths of waves of light with the greatest accuracy, and of analyzing radiations to the degree required for most fundamental investigations. This same instrument, or the principle upon which it is based, also lent itself to an investigation made by Fabry and others, which may be said to be of "practical" importance. All engineering practice involves measurements of length; the universal standard is the meter-bar and its copies. Questions have arisen as to the permanence of this standard, as to whether the metallic bars changed with time; and, in order to have a standard about which no such questions could be asked, it was proposed to measure the meter-bar by means of light-waves. This was first done by our American physicist, Professor Michelson, and later by Fabry, Perot and Benoit. Their results, published in 1913, have once for all determined our standard of length.

In the field of pure science Fabry applied his instrument to the two essential questions of spectroscopy: Accurate measurement of wave-lengths and the investigation of the sources of radiation. This last investigation was in part theoretical, and it has led to a most important relation between the molecular weight of a gas and the character of its radiation. In short, there

is no detail in all the vast domain of spectroscopy which he has not studied and with reference to which he has not notably increased our knowledge. Within recent years his work has dealt with photometry, the science concerned with the intensity of radiation as distinct from its analysis; and here too his contributions to our knowledge and our technique are most valuable.

It was but natural that, when France was plunged into the Great War which threatened her existence and our whole civilization, Fabry should have been called to serve his country in that field in which he was preëminent, the design of apparatus and invention of scientific methods. His work for the Ministry of Inventions was extraordinarily useful, and was so recognized. When, in the early summer of 1917 it was decided to send a mission of scientists to America to help us in our preparation for the war and to acquaint our military authorities with recent technical developments, Fabry was selected to be its leader, and, as Major Fabry, he came to this country in June of that year. Only those familiar with the actual facts can realize the value of the services of this Mission, and one of the outstanding causes of its success was the personality of its chief. It was not his first visit to this country. He knew us and our ways. He possessed all the scientific and technical knowledge required; he was modest, tactful and absolutely without thought of self; but, more important still, he has a sense of humor and an appreciation of our own American variety. He made friends of all, he inspired absolute confidence, and his influence was felt in every detail of our scientific work in the war.

It is particularly fitting that Fabry should be awarded a Medal bearing the name of Franklin. The latter by his visit to France interpreted America to the French people; he presented to them the ideals and the purposes of Americans; it is believed that he was personally liked by the French. So it is with Fabry to-day. He is loved by all Americans who know him, and to every one he is the representative of all that is best and highest in French science. France owes a great deal to him, for he is in every way a worthy son, and our appreciation of French scientific ideals and investigations is exhalted on account of him.

I only wish that you could have the pleasure of seeing him in person to-day. A few of us who have known him for many years have him clearly in our minds. We hear his deep sonorous

voice and his merry laughter; we see his clear, inspiring, sympathetic eyes; we stand under the charm of a great scientist and a true Frenchman. Surely the Midi, the province of his birth, endowed him with those qualities which endear him most to us.

In his absence, however, Mr. President, The Franklin Medal awarded him will be accepted on behalf of his government by His Excellency M. Jusserand, the Ambassador of the French Republic, whom I have the honour to present to you.

The Chairman then said:

Ambassador Jusserand: Permit me, Sir, to assure you that the members of The Franklin Institute realize fully the high distinction which is conferred upon their meeting by the presence this afternoon of the Official Representative of France, our great and greatly beloved Sister Republic.

Your Excellency, The Franklin Institute, upon the recommendation of its Committee on Science and the Arts, has awarded to your countryman, Professor Charles Fabry, The Franklin Medal. This is granted to Professor Fabry in recognition of his distinguished services to mankind rendered in the field of Science. I have the honour, Your Excellency, in the name of the Institute, to hand you The Franklin Medal and Diploma, and a Certificate of Honorary Membership in The Franklin Institute of the State of Pennsylvania, with the request that you transmit them through your government to Professor Charles Fabry, the medalist. You may assure him that this award is the highest honour in the gift of the Institute.

M. Jusserand, in accepting the Medal and Diploma and the Certificate of Honorary Membership, said:

Mr. Chairman, Ladies and Gentlemen: It is with heartfelt appreciation that I accept on behalf of the French Government for Professor Charles Fabry this Medal and the Diplomas which accompany it. They will be duly forwarded and my Government will be informed of what has just taken place, and I hope that there shall be another to add to these honors, one which will not be less appreciated, that is the text of the tribute we have just heard paid Professor Fabry by Doctor Ames. It is so illuminating and so deserving of praise that I am sure that when Professor Fabry has read it he will measure its value with that of the Medal and Honorary Membership in this Institute. I know it will be

a great pleasure for him to think that these honors are bestowed by an Institution bearing the name of the first Minister sent by your country to France, a Minister possessed of the highest combination of scientific and diplomatic attainments.

In reading the works of Franklin there is hardly a page on which we do not find something relating to physical science. Of all the world's great physicists, few have possessed keener critical faculties, greater vision, or a more highly trained imagination. His work in the field of electricity was perhaps the most notable. His discovery of the electrical nature of lightning discharges alone would have made him famous.

While acknowledging the tributes this Institution is paying to Professor Fabry, it is a pleasure to me to note the many honors now being conferred upon his and my fellow countrywoman, Madam Curie, by other great Institutions of learning in America. Her discovery of radium was epoch-making in physical science. Not only has there resulted from this discovery an amazing fund of knowledge relating to the constitution of matter, but in the hands of the physician radium is serving to alleviate and in many cases cure one of the most painful of human maladies. What further secrets the elements may hold and the physicists reveal cannot be foreseen, but it is certain that there are secrets and that they are discoverable only by the scientist possessed of vision and highly trained imagination.

I read with interest in your Journal of last year a paper by Professor Svante Arrhenius on "The World's Supply of Energy." If you have also done so, you will appreciate that the world's coal and petroleum supplies are limited and that eventually other sources of energy must be utilized or discovered if modern industrial civilization is to continue. Among the great forces of nature which we have not yet to any considerable extent harnessed are those of the winds and the tides. Wind power, while great at places and at times, is so inconstant as to be non-commercial; but the utilization of tidal power offers greater promise of success.

I am glad to tell you that a number of favorable situations exist along the French seacoast for the development of the power of the ocean's tides. A remarkable natural site exists in the vicinity of Minihic-St.Suliac, where it is probable that some 40,000 kw. may be continuously produced on a commercial basis.

With the utilization of the power of falling waters and ocean

tides to their fullest much may be done to lessen the demand on the world's coal and oil fields as sources of power. However, should the power from water, coal and oil fail ultimately to meet the needs of industrial civilization, the physicist still holds for us another hope, namely, that of freeing the internal energy of the atom on a commercial scale. Who can predict how soon this may be done?

Mr. W. C. L. Eglin was then recognized and made the following statement concerning the work of Mr. Frank J. Sprague.

Mr. President, Members of The Franklin Institute, and Guests: I have the honour to present Mr. Frank Julian Sprague, to whom, upon the recommendation of the Institute's Committee on Science and the Arts, has been awarded The Franklin Medal, in recognition of his many and fundamentally important inventions and achievements in the field of electrical engineering.

Mr. Sprague was born at Milford, Connecticut, July 25, 1857. He won a competitive appointment to the United States Naval Academy, and was graduated therefrom in 1877. Almost from the time he entered the Naval Academy until he resigned from the naval service, he was especially interested in electrical studies; and during this time he conducted a number of experiments and investigations of applications of electricity in the Navy. His notebooks of that period contain many outlines of inventions which have since been developed and are now applied on naval vessels. He was thrown in contact with the leading scientists and electrical engineers of the world by acting as secretary of a jury composed of eminent scientists at the British Electrical Exhibition held in Sydenham, England, in 1882.

Upon the completion of this work, Mr. Sprague decided to associate himself with Mr. Thomas A. Edison and engage in the development of improvements relating to the electric light distribution system.

About this time, Mr. Sprague conceived the idea of the importance of the developments that were possible by means of the electric motor, and he withdrew from Mr. Edison and organized the Sprague Electric Railway and Motor Company. He succeeded in developing one of the first practical electric motors which was available for all kinds of industrial purposes. His notable work in 1887 for the Union Passenger Railway Company

of Richmond marked the first successful electric street railway system. In this undertaking, Mr. Sprague designed the first complete system including the power house, the transmission and distribution system, and the electrical equipment on the cars; which later became the standard practice for street-car equipment throughout the world. There were many features of fundamental importance in this early development, notably the method of mounting the motor on the axle and the spring suspension between the trucks, which insured the proper alignment between the driving shaft of the motor and the driven wheels; the light trolley wire reinforced by feeders, and the under-running trolley. The work at Richmond provided the necessary information for the solution of the urban street-car transportation problem.

Mr. Sprague, however, continued his work on other inventions and developments of apparatus, and devised means for the electrical operation of both elevated and subway trains through a system known as "multiple-unit control," whereby a master controller is capable of operating any number of cars either with or without motor equipment, in the form of a continuous train, with a single operator, this system being equally applicable to two or more locomotives.

Mr. Sprague has been a pioneer in the invention and development of transportation for both freight and passengers, for all classes of railroad service, and to him is due the great development which has been made in the progress of the art.

The work of Mr. Sprague in connection with street railways and elevated and underground railroads naturally turned his attention to the electrical operation of elevators, and here again his marked inventive and constructive ability made possible the electric elevator which is now almost universally used for both passenger and freight purposes.

The development of the electric motor and its application to industrial purposes and in the art of electric traction, have done much to increase the establishing of world-wide industries and have greatly helped in promoting human welfare. Mr. Sprague has had the good fortune to live to see his discoveries and inventions applied to this art of electric traction throughout the world.

The Institute in 1903 awarded to Mr. Sprague the Elliott Cresson Medal, and now we are happy to be able to present him to you for the award of The Franklin Medal and the accompanying

Certificate, and the Certificate of Honorary Membership in the Institute—the highest honours it can bestow.

The Chairman, in presenting The Franklin Medal and Diploma and Certificate of Honorary Membership to Mr. Sprague, said:

Mr. Sprague: I have the honour in the name of The Franklin Institute and on the recommendation of its Committee on Science and the Arts and in recognition of your distinguished services to mankind rendered in the field of applied science, to present to you The Franklin Medal and Diploma, with a Certificate of Honorary Membership in The Franklin Institute of the State of Pennsylvania.

This is the highest honour in the gift of the Institute.

Mr. Sprague, in accepting the Franklin Medal and Diploma and the Certificate of Honorary Membership, said:

Mr. Chairman, Members and Guests: I would not be quite human if, on an occasion of this kind, when receiving from the oldest and most dignified scientific institution of this country so notable a mark of its consideration, in the award of the medal bearing the name of the great Franklin and the adding of my name to the illustrious list carried on your Honorary roll, I did not feel both a sense of pride and humility—especially as my work has been less that of the physicist than of one who has endeavored to make use of the fundamental facts discovered by patient delvers into the mysteries of the physical world.

Mr. Eglin in his kind references to me has betrayed my age, perhaps as an evidence of many years of work, but I feel somewhat solaced by the fact that, in the presence of the Ambassador of that country for which we have so close a friendship and of the General who was at the head of our armies operating there during the late war, a like award is also extended to Professor Fabry, a distinguished son of France and one of the greatest living scientists. It has made me feel that the ties between our countries are even more closely knit.

I cannot regard this honour entirely as an individual one, but to a great degree as one granted also to those who have been associated with me and with the development of electric art, so many of whom have had no personal recognition. Fortunately for me I have lived to receive such.

In every specific advance of human endeavor it is at least convenient, and customary, to choose some name as a sort of reference mark, the work of such a one being something like a hub around which other perhaps equally important work revolves, and it is, of course, a matter of great personal satisfaction that mine has been considered of sufficient importance to merit this distinction.

As an ex-Naval officer, I am glad also for my alma mater, the U. S. Naval Academy, and I wish to take this opportunity to say that I am one of those who believe that our schools, colleges and universities, however much they sometimes may be criticized, are the institutions which, because of their laying of the basis of mental training, largely make the men of the country. A few years ago an engineer of a great manufacturing company, in addressing his brother engineers at Manchester, England, in reply to the query as to what in his opinion was the most important influence in the development of the electric art, replied in effect that while his first answer would have been the discovery of three enumerated inventions he had come to the conclusion that it was the introduction of the University man into it.

As one surveys the great body of men now engaged in responsible fashion the force of this admission is evident, and as a testimonial to the importance of proper technical training it is important to note that it is with great difficulty that a young man without a University training can enter even the student courses of the more important electrical corporations.

When I look at this medal I am wondering whether it is after all quite real, as we of past generations understand realities, for because one of our sister planets has exhibited slight vagaries in her annual peregrinations, and possibly for some other reasons which I do not understand, our old friends Newton and Euclid seem no longer secure on their pedestals, for we are informed by a distinguished mathematician that three dimension measurements must now be corrected by a time element.

According to this new, and to my practically inclined mind somewhat abstruse theory, this medal when I move it from me outwards changes its dimensions somewhat. But on consideration I must after all consider it a very real thing, because I call to mind the fact that unless it is moved with a velocity approaching that of the maximum thus far observed and reputed impos-

sible of increase, the speed of light, the fourth dimension represented by the time element, is of no particular importance.

Possibly it indeed may be found that there is not much new even in the electrical field after our friends of the American Philosophical Society have finished deciphering the strange hieroglyphics of a famous monk of the early centuries, one Roger Bacon. It is strange how the name of Bacon arises to trouble us, first as one through whom the authorship of our greatest dramatic writings is assailed, and now of another who may be found to have antedated so much of modern discovery.

However, I shall not allow myself to be disturbed by these scientific and historical possibilities, but cling with such tenacity as I may to this evidence of present honour, for which I again deeply express my thanks.

Dr. Joseph S. Ames then read the paper on "Studies in the Field of Light Radiations," prepared by Professor Charles Fabry for the occasion, and Mr. Sprague presented a paper on "Electric Traction—A Review." [1]

The Second Royal Society Conversazione for 1921. (*Nature*, June 2, 1921.)—Sir John Dewrance and Prof. E. G. Coker exhibited an arrangement for investigating the stresses arising from the use of turning tools. Both tool and work were made of transparent material and polarized light was transmitted through them. From the effect produced upon the light the stresses could be derived.

The Meteorological Office had on exhibition an apparatus for recording atmospheric pollution. Every 15 minutes two litres of air are drawn through a piece of blotting paper. From the darkness of the deposit is inferred the amount of suspended matter. Records showed the reduction owing to the coal strike. A close connection was shown between the vertical electrical force and the amount of suspended matter.

From the National Physical Laboratory came a Paterson-Walsh electrical height finder by which is obtained a continuous indication of the height of air craft.

The Science Museum showed an Eötvös gravity torsion balance for investigating the variation of gravity over small distances. This has been employed in Hungary for locating deposits of ore whose density differs considerably from that of its surroundings.

G. F. S.

[1] These papers appear on pages 277 and 291, respectively, of this issue.

NOTES FROM THE U. S. BUREAU OF STANDARDS.*

A PORTABLE VACUUM THERMOPILE.[1]

[ABSTRACT.]

A DESCRIPTION is given of a portable vacuum thermopile, of glass, in which a calcium evacuator is permanently attached to the container. The paper gives general directions for operating the device, as well as data on the thermopile, which is of bismuth-silver.

Observations extending over a period of about seven years are given on the behavior of vacuum thermopiles in which the vacuum is maintained by means of calcium, which has the property of combining with gases when it is heated.

INTERFERENCE MEASUREMENTS IN THE SPECTRA OF ARGON, KRYPTON, AND XENON.[2]

By F. W. Meggers.

[ABSTRACT.]

THE spectra of the inert gases, especially of the heavier ones, contain strong lines which represent wave-lengths possessing a high degree of homogeneity and reproductibility and these lines are therefore suitable for use as standards of wave-length. This paper gives the values of 50 wave-lengths in the spectrum of argon (3948 to 8521 A), 18 of krypton (4273 to 7601 A) and 12 of xenon (4500 to 4923 A), all of which have been compared with the wave-length of the red radiation from cadmium (6438.4696 A) which is the international primary standard. These wave-length comparisons were made by means of etalon-interferometers and most of the values are probably correct to one part in several millions. The elegance and precision of the interferometer methods for wave-length comparisons demonstrated by the close agreement between values obtained by different observers, and also by the constant

* Communicated by the Director.

[1] Scientific Paper No. 413.

[2] Scientific Paper No. 414.

frequency differences of many of the lines belonging to combination series. There are only a few cases where independent observers differ by 0.004 A or more. From the wave-length measurements in argon and krypton, frequency differences are obtained which are constant within the probable error of the measurements. This further confirms the exactness of the Combination Principle of Ritz. If these frequency differences are regarded as true constants they testify to the accuracy in relative value of the wavelengths involved.

A PRELIMINARY STUDY OF TEARING INSTRUMENTS AND TEARING TEST METHODS FOR PAPER TESTING.[1]

By Paul L. Houston.

[ABSTRACT.]

IN this technologic paper a study is made of the relative effect of different sizes of test samples on the tearing strength of paper. A great number of samples of commercial papers are torn on three different instruments, using different sizes of test samples and also the same sizes of test samples. Data are collected, accordingly, to show that the larger the test samples, the greater are the values of tearing strength. The reason for this is brought out as fabric assistance, which is of considerable importance in the textile industry.

The three instruments used in this study are a tensile strength instrument and two types of instruments for determining the tearing strength of paper. These two types of instruments are called type I and type II. Type I is a recording instrument, while type II is a non-recording instrument. A study is then made of these two types of tearing instruments for the purpose of investigating their accuracy and reliability, so that the results of this investigation may benefit the paper industry.

It is found in this study that it is impossible to calibrate the type I recording instrument because of the friction in the pin-slot bearing of the recording arm and the friction between recording pen and paper chart. Also, in a performance test the recording instrument does not check itself on the same grade of paper within the variation of the strength of the fibres themselves. On the other

[1] Technologic Paper No. 194.

hand, it is found that the type II non-recording instrument is accurate within 5 per cent. on the majority of papers, with the exception of those writings and printings that are of lighter weight than 50 lb. to the sandard size ream, $25 \times 40 - 500$. This instrument does also check itself in a performance test on the same grade of paper within the variation of the strength of the fibres themselves.

Conclusions are drawn up to show that the type I recording instrument is not an accurate and reliable piece of apparatus, and that the type II non-recording instrument is the more reliable of the two, and is sufficiently accurate and reliable for a mill test on the majority of papers, but not sufficiently accurate for a laboratory test.

Sour Salt.—CHARLES H. LA WALL (*Am. J. Pharmacy*, 1921, xciii, 496–497) states that "sour salt" is a synonym for tartaric and citric acids. These acids are sold under the name of sour salt as a condiment, and are used to reinforce vinegar in imparting acidity to certain foods. The "salt" always occurs in commerce in the crystalline form. It has been adulterated by substitution of alum crystals for a part or all of the organic acids. J. S. H.

Reaction between Sodium Carbonate and Chrome Alum.—According to LOUIS MEUNIER (*J. Amer. Leather Chemists' Asso.*, 1921, xvi, 321–327), chromic hydroxide is precipitated when sodium carbonate acts upon a cold, freshly prepared solution of chrome alum. The precipitate is a basic chromic sulphate when sodium carbonate acts upon a very old solution of chrome alum, or when precipitation occurs at a temperature of 100° C. The sodium carbonate neutralizes the sulphuric acid, which has been formed by hydrolysis of the alum in aqueous solution, and also coagulates the colloidal chromic hydroxide or basic sulphate which have been formed in the aqueous solution. J. S. H.

Potable Water and Its Hydrogen Ion Concentration.—For the determination of the hydrogen ion concentration of drinking water, J. M. KOLTHOFF, of the University of Utrecht (*Zeit. Unters. Nahrungs- und Genussm.*, 1921, xli, 112–122), prefers the indicator method to the hydrogen electrode. Neutral red is used as the indicator. The hydrogen ion concentration is a minor factor in judging a drinking water. However, if it and the bicarbonate content be known, the total carbonate content may readily be calculated.

J. S. H.

Talc and Soapstone. (*U. S. Geological Survey Press Bulletin No. 476*, August, 1921.)—Although talc is a mineral that is most widely known in the form of talcum powder, it is extensively used in the industries. The pure mineral is known as talc and the massive rock that contains it is known as soapstone. Nine-tenths of the talc and soapstone mined is ground and used as a filler in paper and in rubber, as foundry facing, and in many other ways.

The United States is by far the greatest producer of talc and soapstone, and it consumes even more than it produces. In 1919 it produced 68 per cent. of the world's supply and consumed 79 per cent.

The production of talc and soapstone in 1920 exceeded that in any previous year both in quantity and in value, according to Edward Sampson, of the United States Geological Survey, Department of the Interior. The sales in 1920 amounted to 224,290 short tons, valued at $3,090,265, an increase in 1920 over 1919 of 21 per cent. in quantity and 31 per cent. in value. The quantity exceeded by 2 per cent. that of 1917, the previous record-quantity year, and the value exceeded by 15 per cent. that of 1918, the previous record-value year.

The quantity of talc reported to the Geological Survey as ground was 178,505 tons, valued at $2,142,894, or 18 per cent. more than in 1919. The value of the ground talc was the highest on record, exceeding that for 1919 by 34 per cent. and that for 1918, the previous record-value year, by 15 per cent. The average price of ground talc in 1920 was the highest on record, namely, $12 per short ton, which may be compared with $10.55 in 1918 and 1919 and $8.42 in the prewar year 1913.

The manufactured soapstone sold amounted to 19,707 tons, valued at $709,400, not the highest annual output recorded, but the highest annual value.

Silica Brick Industry. (*U. S. Geological Survey Press Bulletin No. 476*, August, 1921.)—The refractory silica brick industry in the United States continued to make progress in 1920. These brick are used principally in by-product coke ovens, in open-hearth steel furnaces, in copper reverberatory furnaces, and in the glass industry, in which they must withstand high temperatures, such as would fuse ordinary clay fire brick. They also withstand abrasion well. The output in 1920, as estimated by Jefferson Middleton, of the United States Geological Survey, was 255,000,000 brick, valued at $15,540,000, or $60.94 a thousand, an increase of 18 per cent. in quantity and 32 per cent. in value as compared with 1919. The quantity marketed in 1920 was exceeded by that in 1917 and in 1918, when the stress of war caused an increase in the production of all refractories. The value in 1920 was exceeded only by that in 1918. The price per thousand in 1920 was the highest recorded. The output in 1920 was 46 per cent. greater, the value 307 per cent. greater, and the average price per thousand 178 per cent. greater than in 1913.

NOTES FROM THE RESEARCH LABORATORY EASTMAN KODAK COMPANY.*

THE INTENSITY OF SCATTERED X-RAYS IN RADIOGRAPHY.[1]

By R. B. Wilsey.

[ABSTRACT.]

In radiographing through the thicker portions of the human body, scattered X-rays have a considerable effect in fogging the image and thereby reducing the contrast and definition. In this investigation, a volume of water was used as the scattering material, inasmuch as its absorption and scattering characteristics are similar to those of human flesh. The relative photographic intensities of the scattered and primary radiation reaching the film were measured under a variety of conditions, showing the effects of thickness of scattering materials, sizes of cones or diaphragms between the tube and the scattering material, tube voltage, and filters placed between the scattering material and the film.

Under conditions similar to those in the radiography of thick parts, the scattered radiation reaching the film is roughly from four to ten times the intensity of the primary radiation. The intensity of the scattered radiation is small when the thickness of the scattering material is small, or when the X-ray image is diaphragmed to a very small size.

The removal of any scattered radiation necessitates an increase in exposure to compensate for the loss in total X-ray intensity. The exposure factors corresponding to various thicknesses of water and image sizes were computed from the experimental data.

The ratio of diffuse to primary focal radiation reaching the film was found to increase slightly as the tube voltage was increased.

Filters placed between the scattering material and the film diminish slightly the proportion of diffuse radiation. The filters are of no practical value, however, inasmuch as they do not improve radiographic contrast.

* Communicated by the Director.

[1] *Am. Jour. of Röntgen.*, **8**, June, 1921, p. 328, Communication No. 102 from the Research Laboratory of the Eastman Kodak Company.

THE MUTUAL ACTION OF ADJACENT PHOTOGRAPHIC IMAGES.[2]

By Frank E. Ross.

[ABSTRACT.]

A REVIEW of the work of Kostinsky, Lau, Turner, Bellamy and others on the attraction and repulsion of neighboring star images was given. The factors which control the action were shown to be (1) *turbidity*, causing an attraction through optical reinforcement of the images on their adjacent sides. A formula was derived giving the amount of this attraction. (2) *Gelatine* disturbance, causing an attraction also. (3) *Developer* action, in which development of the images on their adjacent sides is retarded, owing to the products of reaction, as in the Eberhart effect. This leads to a repulsion. In the case of neighboring absorption spectral lines there is an additional effect due to a difference in sharpness of the edges which is caused by turbidity and halation.

Experimental data on the behavior of artificial double stars, close bright lines, and close absorption lines were described. It was shown that in experiments of this nature important differences develop depending on whether the exposures are normal or over-exposed. The contradictory results obtained by various investigators are thus explained. If the behavior is investigated by means of over-exposure, strong repulsions of neighboring images are found except in the case of absorption lines in which a strong attraction is found. In the case of normal exposure, which is of most interest in general, an attraction is usually obtained amounting to two or three microns. In order to reduce the affect, which enters as an error of importance in measurements of this kind, *the exposure must be reduced to a minimum.* In the case of over-exposures, considerable variation of the repulsive action was obtained by varying the developer. Curves for a number of developers were given in which it is shown that an attraction sets in, followed by a repulsion as the distance becomes less. It is important to make a further study of developers.

[2] *Astro. Jour.*, vol. liii, no. 5, June, 1921, p. 349. Communication No. 119, from the Research Laboratory of the Eastman Kodak Company.

ELECTROLYTIC MANUFACTURE OF p-AMINOPHENOL.[3]

By A. S. McDaniel, L. Schneider, and A. Ballard.

[ABSTRACT.]

NITROBENZENE was reduced electrolytically at 30° C. in strong sulphuric acid (90-91 per cent.), free from iron and heavy metals, using platinum electrodes, with a cathode current density of 6–8 amperes per 100 square centimetres, and anode density double. Glazed earthenware cells with porus diaphragms of thin, dense porcelain were used. Under these conditions minimum sulphonation was obtained. By washing the cell sludge with commercial hydrochloric acid, centrifuging, and treating with pure hydrochloric acid p–aminophenol hydrochloride was obtained, the yield being 40–50 per cent. on the nitrobenzene (exclusive of mother liquors which yield a further 10 per cent.). Apart from the cost of platinum, the most important economic factor is the necessity of providing for the cheap concentration of the spent acid. Cost of upkeep, owing to the excessive corrosive action of the matrials on the apparatus, is also considerable.

Atomic Weight of Germanium.—(Correction.) John H. Müller of the University of Pennsylvania has determined the atomic weight of germanium by heating a known mass of potassium fluogermanate in a current of hydrogen chloride, and weighing the residual potassium chloride (*Jour. Am. Chem. Soc.*, 1921, xliii, 1085–1095). Seven determinations gave, as an average, an atomic weight of 72.418 for germanium. J. S. H.

Prof. C. Runge, of Goettingen, has sent to *Nature* a list of men of science in Germany who have died since the beginning of the war. Among them are: Physicists—Hittorf, Riecke, E. Mach, Helmert, W. Voigt and Elster; chemists—E. Fischer and A. von Baeyer; mathematicians—Lexis, R. Dedekind, Frobenius, Cantor, Staeckel, Thomae and T. Reye; astronomers—von Auwers, Schwarzschild, Bruns, and W. Foerster; botanists—Voechting, Solms-Laubach, W. Pfeffer, whose investigations led to the conception of osmotic pressure, and Schwendener. G. F. S.

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[3] *Trans. Amer. Electrochem. Soc.*, 1921, p. 319–327, Communication No. 69, from the Research Laboratory of the Eastman Kodak Company

The Time Interval between Absorption and Emission of Light in Fluorescence. R. W. WOOD. (*Proc. Royal Society*, A 700.)—When ultra-violet light of short wave-length is absorbed by freshly formed mercury vapor the latter fluoresces with a bluish green light. That an interval elapses between the absorption of light and the appearance of the fluorescence is shown by the following experiment. A bent tube of fused quartz containing a little liquid mercury is exhausted and sealed. That leg of the tube in the lower end of which is the mercury is supported so as to be vertical. A flame placed beneath causes the mercury to distil into the distant end of the tube. Light from an aluminium spark passed through a hole in a metal screen and traversed the vapor in the tube. Except at the walls where the speed of the rising vapor was small, there was no trace of fluorescent light in the part of the tube where the light passed through the vapor, but two or three millimetres above the green light appeared. The time required for the vapor to rise through this short distance is the interval between excitation and appearance. To measure the time more accurately a phosphoroscope was specially designed. By its use the interval in the case of mercury vapor was found to be about one fifteen-thousandth sec.; for uranium glass, less than one four-hundred-thousandth sec. The duration of the phosphorescence of barium platino-cyanide came out equal to only one four-hundred-thousandth sec., while for fluorescence the duration was less than .00000044 sec.

Fluorescene, eosine, rhodamine, etc., are transformed into non-fluorescing bodies with absorption bands different from those of the parent substance. All fluorescing solutions are made from organic compounds with the single exception of concentrated solutions of some uranyl salts. "The idea occurs to one as to whether the phenomenon may not perhaps be associated with the natural break-down of uranium." G. F. S.

Asphalt Production in 1920. (*U. S. Geological Survey Press Bulletin No. 476*, August, 1921.)—The quantity of native asphalt and native bitumens sold in the United States in 1920 was 198,497 short tons, valued at $1,213,908. This was an increase of 125 per cent. in quantity and of about 78 per cent. in value over 1919. Gilsonite was reported from Uinta County, Utah, wurtzilite (or elaterite) from Duchesne County, Utah, and grahamite from Pushmataha County, Oklahoma.

The sales of manufactured asphalt obtained from domestic petroleum amounted to 700.496 short tons, valued at $11,985,457, or $17.11 a ton. Compared with 1919 these figures indicate an increase of 14 per cent. in quantity and 37 per cent. in value.

The sales of asphalt manufactured in the United States from Mexican petroleum in 1920 amounted to 1,045, 779 short tons, valued at $14,272,862, or $13.65 a ton. This was an increase of 55 per cent. in quantity and of 85 per cent. in value over 1919.

NOTES FROM THE U. S. BUREAU OF CHEMISTRY.*

STUDIES IN NUTRITION. VIII. THE NUTRITIVE VALUE OF THE PROTEINS OF TOMATO-SEED PRESS CAKE.[1]

By A. J. Finks and Carl O. Johns.

[ABSTRACT.]

A DIET in which tomato-seed press cake furnished the sole source of protein and water-soluble vitamine enabled albino rats to grow at the normal rate. Such a diet was made adequate by the addition of starch, a suitable inorganic salt mixture, butter-fat, and lard. Normal growth was also obtained when the butter-fat of the above diet was replaced by lard.

THE APPLICATION OF OPTICAL METHODS TO THE EXAMINATION OF INSECTICIDES AND FUNGICIDES.[2]

By George L. Keeman.

[ABSTRACT.]

ATTENTION has been called to the application of optical-crystallographic methods for the identification of crystallizable chemical salts in insecticides and fungicides. The Bureau has found the use of such methods of value in many instances. A single method of procedure, particularly suitable for microanalysts who have not had extensive training in crystallography and mineralogy, has been outlined.

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* Communicated by the Acting Chief of the Bureau.

[1] Published in *Am. J. Physiol.*, **56** (1921), 404.

[2] Published in *J. Am. Pharm. Asso.*, **10** (1921), 336.

THE MINERAL CONSTITUENTS OF POTATOES AND POTATO FLOUR: EFFECT OF PROCESS OF MANUFACTURE ON COMPOSITION OF THE ASH OF POTATO FLOUR.[2]

By C. E. Mangels.

[ABSTRACT.]

SAMPLES of fresh potatoes from different sources show differences in the amounts of mineral constituents present. Potato flour contains a smaller relative amount of total ash, and a very slightly smaller percentage of potash than the corresponding fresh potato. The relative distribution of different mineral constituents in the ash is not appreciably changed during the process of manufacture of the flour. Insofar as mineral constituents are concerned, the nutritive value of potato flour is practically the same as that of the fresh potato.

Annual Meeting of the French Society of Industrial Chemistry.—The newly formed Society of Industrial Chemistry in France will hold a meeting from October 9th to 12th, both dates inclusive, at the Conservatory of Arts and Trades in Paris. The opening session will be held under the chairmanship of Mr. Dior, Minister of Commerce. Thirty-four sections, covering all departments of applied chemistry will be provided, and many of the contributions will be by eminent specialists. The sessions for scientific business will close on October 11th at 5 o'clock, on which occasion the chair will be occupied by Mr. Louch Eur, minister of the recently evacuated regions. In the evening a banquet will be given at the Palais d'Orsay, under the presidency of Mr. Lefebvre du Prey, Minister of Agriculture. The last day will be occupied by excursions to manufacturing establishments. From October 7th to 16th an exposition will be held in the Conservatory of Arts and Trades, under the auspices of the society, which exposition, being merely initiative, will consist of but two sections: Apparatus for laboratory use and industrial control; dyestuffs. Many French establishments have already signified intentions to exhibit. H. L.

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[2] Published in *J. Ind. Eng. Chem.*, 13 (1921), 418.

NOTES FROM THE U. S. BUREAU OF MINES.*

LABORATORY STUDIES OF THE TRENT PROCESS.

By O. P. Hood.

DURING the war certain suggestions concerning power production were made by Mr. Walter E. Trent to the War Inventions Board, and at the request of the War Department, facilities for experimental work were provided on the grounds of the Bureau of Standards. The experiments were along the line of controlling the conditions of combustion in a closed space. In order to reduce slag troubles, experiments were carried out for removing ash from powdered coal. After the war, work along this line was continued, resulting in the Trent Process, which agitates or beats together powdered coal, water and oil. A new technology had previously been given to ore preparation by the use of small quantities of oil in water with froth flotation, and although the methods, results, and mixtures of the Trent process were quite different, yet the same physical phenomena of differential wetting were used, and the possibility of there being interesting results in full technology was evident. A coöperative agreement was entered into, whereby the Bureau of Mines was to investigate the underlying physical and chemical facts and make them public, and the Trent Corporation was to pay the cost of the investigation.

The several reports as made have been available to any one interested, and are now to be published. While the Bureau of Mines felt justified in investigating the physical phenomena so far as might be done in a laboratory, and so far as public interest might reach, no attempt was made to discover the commercial possibilities which development might bring. The question of commercial possibilities must be left for commercial enterprise to answer.

Briefly, the process consists in agitating together powdered coal, water and oil. This produces a partly de-ashed plastic fuel, called

* Communicated by the Director.

an amalgam, the oil selecting the coal particles and largely excluding the water and ash. The amalgam can be freed from water mechanically held by working much the same as butter is worked. The amalgam can be burned in several ways; it may be shoveled, or forced through pipes by pressure; it can also be stored, under water if desired.

The laboratory results immediately suggest many interesting possible applications: (1) For pulverizing fuel wet grinding presents many advantages over dry grinding, provided the water can be eliminated afterwards; (2) to be able to reduce the ash in coal may make available great quantities of low-grade coals and material now considered as waste at the mines; (3) if an oil is used which can be distilled at a temperature below the distilling temperature of the coal, powdered fuel is reclaimed from the amalgam and the oil may be reused. If a heavy oil be used and distilled to dryness, a coke product may be recovered, although the coal used may have had no coking quality. If the distillation proceed only to a heavy pitch, a mass suitable for briquetting may be made; (4) in distilling oil mixed with a finely powdered material, the distillates are similar to those obtained by distilling under pressure, so that the distillation of an amalgam of coal and oil gives quantities often more favorable than the sum of the separate distillations of the coal and the oil; (5) the amalgam can be used for a gas-making fuel, and gas-house tar emulsions can be dehydrated by mixing with powdered coal, the amalgam being retorted for further gas making; (6) graphite ore can be separated from its gangue, and coke can be separated from flue dust by using the Trent process. Clean coal in anthracite sludge will make an amalgam if oil is added.

The brief sketch of possibilities revealed by small-scale laboratory work shows that the field for investigation and development is large. The general results show that real benefits are physically possible by treating coal in this manner. The Bureau has interested itself more particularly in the ash separation phenomenon or the cleaning of coal, and in the distillation of the amalgam. Details of the experimental work on ash separation are presented in a recent report, "The Trent Process of Cleaning Coal," by G. St. J. Perrott and S. P. Kinney. Results of the distillation work will be discussed in a future article.

SEPARATION OF SPHALERITE, SILICA AND CALCITE FROM FLUORSPAR.

By John Gross.

SOUTHERN Illinois and Kentucky are credited with approximately 90 per cent. of the fluorspar production of the United States. A small amount of high-grade acid spar is obtained from the ore by hand sorting; the greater part, however, is recovered by jigs and tables as gravel spar containing approximately 85 per cent. calcium fluoride. In the jigging and tabling operations, galena is recovered, and enough silica and calcite are removed to raise the calcium fluoride content of the gravel spar to 85 per cent. However, the removal of silica and calcite results in a large loss of spar. With increasing depth in the mines, zinc sulphide, sphalerite, is encountered in increasing quantity and the removal of this has become a serious problem. While the sphalerite can be fairly well removed from the spar by present milling methods, a marketable zinc concentrate can not be made.

In experiments made on fluorspar ore from Southern Illinois, by the mining experiment station of the U. S. Bureau of Mines, at Rolla, Missouri, in coöperation with the Missouri School of Mines and Metallurgy, two methods of treatment were worked out. One involves electrostatic separation, and the other flotation. Details of the methods are given in a short report recently issued by the Bureau.

LEACHING IRON ORES FOR PHOSPHORUS NOT PRACTICABLE.

By R. M. Winslow.

THE phosphorus content of an iron ore is a determining factor as regards the value of an ore and its metallurgical treatment. The obvious advantages that would pertain to a cheap method for removing phosphorus by leaching has led the Bureau to make some laboratory tests to determine whether it is economically possible to reduce the phosphorus content appreciably by leaching. Sulphuric acid proved the best solvent, but the highest extractions were negligible from a commercial standpoint. The conclusion is that the method is not commercially feasible. Further details will be found in a short paper recently published by the Bureau.

The Mass Spectra of the Chemical Elements. F. W. ASTON. (*Phil. Mag.*, July, 1921; *Nature*, June 23, 1921.)—By continuing his experiments with the mass spectrograph the Clerk Maxwell Student of the University of Cambridge causes the positive rays to reveal that the element nickel is made up of two isotopes, the stronger line coming from a constituent of atomic weight 58, and the weaker from one of atomic weight 60. "The intensities of the lines are about in the ratio 2:1, and this agrees with the accepted atomic weight 58.68." In the discharge tube vapor of nickel carbonyl mixed with carbon dioxide was used.

When selenium in the compound selenium hydride and tellurium as tellurium methyl were investigated complete failures resulted. "The failure is unfortunate in the case of Te on account of its well-known anomalous position with respect to I in the periodic table; in the case of Se particularly so for the following reasons: If the accepted atomic weight is even approximately correct this element must have one isotope at least of atomic weight greater than 78. But the numbers 79, 80, 81, 82, 83, 84 are already filled by isotopes of Br and Kr, so that it is extremely probable that one of the isotopes of Se has an atomic weight identical with one of an element having a different atomic number, *i.e.*, is an isobar. The latter are known to exist among radioactive elements, but none have so far been discovered during the work on mass spectra."

Iodine, investigated in the form methyl iodide, gave one line only and this a strong one, corresponding to atomic weight 127. "This proves iodine to be a simple element in an unequivocal manner, a rather unexpected result since all the speculative theories of element evolution, by Van den Broek and others, predict a complex iodine. Kohlweiler not only deduces from theory five isotopes of iodine, but also claims to have achieved a considerable separation of them by diffusion." Antimony and tin gave no definite results. Xenon was tried again under more favorable conditions. The results corroborate the existence of the five isotopes previously announced and suggest the possibility of two additional ones.

By experiments on negatively charged chlorine it is concluded that the isotopes of this element have the atomic weights 35 and 37, while there may be a further one, 39.

The following remark of the author throws light upon the difficulties he is encountering and overcoming. "The particular method of generating positive rays employed by means of a large discharge tube is only suitable for investigating elements which have themselves a reasonably high vapor pressure or are capable of forming compounds which possess that necessary property. To the first group belong the elementary gases, to the second such elements as carbon and boron. Unfortunately the majority of the elements, including all the metals except mercury, do not satisfy these conditions to any great extent. It is therefore natural that as elements less and less suitable were employed the work grew progressively more and more difficult and the results either inconclusive or entirely negative."

G. F. S.

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LIBRARY NOTES.

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BOOK NOTICES.

ORGANIC COMPOUNDS OF MERCURY. By Frank C. Whitmore, Ph.D., Northwestern University. 8vo., 380 pages and index. The Chemical Catalog Company, Inc., New York, 1921.

This is one of the series of monographs, issued under the editorship of the American Chemical Society, a series which will be of great value to American chemists. The organic compounds of mercury have been extensively studied from the point of view of pure science, but this study has acquired in recent years a practical phase in consequence of the desire for a mercurial compound which does not ionize under ordinary conditons and may, therefore, be usable in comparatively large doses in specific diseases, without involving the well-known poisonous actions of the common mercury salts.

The whole field of organo-metallic chemistry is interesting especially in its complexity. One of the earliest compounds of the class was the substance long known as "Cadet's fuming liquor," obtained empirically in 1760 by the Paris apothecary whose name it bears. Bunsen showed many years later that the principal ingredient is a compound of methyl, arsenic and oxygen, and he gave to the arsen-methyl group the name "kakodyl." His wonderful series of researches showed the wide range of combinations possible, and led, among other results, to the discovery of kakodylic acid, with its interesting property of much lower toxicity than the content of arsenic would indicate. At that time the relation of ionization to chemical activity was not known.

Doctor Whitmore points out that the arsenical compounds have received much more publicity than the mercurials, due probably to the prominence which the former have both in therapeutics and toxicology. The preparation of the arsenical compound dates from the eighteenth century, but the first production of a recognized organo-mercury compound appears to have been in 1843, when A. W. Hofman obtained one from the reaction of aniline and mercuric chloride. The exact structural formula of this is not known. Definite production of the mercury compounds dates from 1850, when Frankland noted the reaction of mercury on ethyl iodide and two years later ascertained that methyl iodide gives with mercury under the influence of sunlight, a methyl mercury iodide, CH_3HgI. The earlier steps in the development of the chemistry of these compounds are indicated in the book, and one is impressed with the immense number of the known forms and their complexity. As might be expected, most of them are poisonous, some powerfully so. In fact the discovery of mercuric methyl and mercuric ethyl gave rise to some very acrimonious correspondence in the pages of the *Chemical News* (1865, xii; 1866, xiii) owing to the fatal poisoning of two assistants in the laboratory of St. Bartholomew's Hospital in London, in which the original investigations by Frankland and afterwards Odling were carried out. Doctor Ulrich, a German chemist, and Doctor Sloper, an Englishman, suffered from chronic poisoning as a result of considerable work in connection with the preparation of the substances, and Doctor Phipson accused Doctor Frankland of having sacrificed his assistants. The charges and the

counter-charges were continued for some months, but it does not appear that the principals, Frankland and Odling, were in the least culpable. At the time these two compounds were first prepared, they were among other properties distinguished by their high refractive and dispersive powers, and it was suggested that they might be available for filling hollow prisms. Their extreme poisonous qualities would surely render them unavailabe for such use.

Doctor Whitmore's book is a most valuable addition to the literature of a special class of compounds, and exhibits on every page the painstaking, laborious search of an extensive literature and the arrangement of a great mass of facts in a convenient and vivid form.

HENRY LEFFMANN.

A COURSE IN QUALITATIVE CHEMICAL ANALYSIS OF INORGANIC SUBSTANCES. By Olin Freeman Tower, Ph.D., Adelbert College. Fourth edition, revised, 8vo., xv-90 pages. Philadelphia, P. Blakiston's Son & Co. $1.50 net.

This being the fourth issue of the work it is evident that it has had success. It contains in the main the usual procedures for the separation of the metals into groups by the familiar group-reagents, and the subsequent identification of individuals by special tests. The introduction of the ionic theory has involved considerable explanation of the principles of many of the reactions, so that the modern manuals are less empiric than those of fifty years ago. The preliminary qualitative analysis scheme has long been in favor with teachers, but it is a question whether it might not be substituted by something having a more distinctly quantitative character. The book presents the subject in pretty full detail and gives numerous explanations.

HENRY LEFFMANN.

DAIRY BACTERIOLOGY. By Orla Jensen, Ph.D., Polytechnic College, Copenhagen. Translated by P. S. Arup, B.Sc., F.I.C. 8vo., xii-174 pages, index and 70 illustrations. Philadelphia, P. Blakiston's Son & Co. $3.00 net.

Denmark is one of the great dairy countries of the world, and Danish chemists have contributed very largely to the progress of the science. We may be sure, therefore, that this book will contain a large amount of information on the field to which it is devoted. There is no problem in general hygiene more important and more difficult to handle than the milk supply for cities. The ease with which moderate adulteration may be practised, and the temptation thereto, coupled with the liability of the material to acquire injurious conditions without specific indication thereof, has made the regulation of the milk supply one of the most formidable questions in municipal management. A large part of this management is in the control of the cleanliness of the milk, using the word in its widest sense. The bacteriology of market milk has been extensively studied, and has been found very complex. The work in hand begins with an account of the general morphology of minute organisms, classifying

"micro-organisms" as living forms that cannot be seen by the unaided eye. Leeuwenhoek is, as is customary, credited with the discovery or, at least, the first recorded observation of bacteria, but it is somewhat doubtful whether he saw any of the organisms that are now included under that title.

The work is commendable. It cannot be said that it is superior to American works already available, but it is fully up to the required standard. Originating in Denmark and translated in England, there is a liability to an insufficient presentation of American standards and practises. An instance of this is found on page 88, where it is stated that in order to avoid marketing defective tins of condensed milk, the manufacturer should retain the product for a short time. American manufacturers of canned goods have automatic apparatus for detecting leaky cans, and have no trouble from that source.

All those interested in dairy bacteriology will find a large amount of useful information, well expressed, in this book. It is well printed and quite liberally illustrated.

HENRY LEFFMANN.

A TEXT-BOOK OF ORGANIC CHEMISTRY. By Joseph Scudder Chamberlain, Ph.D., Massachusetts Agricultural College. Small 8vo., xliii–927 pages and index. Philadelphia, P. Blakiston's Son & Co. $4.00 net.

This work is in the form of the ordinary text-books of organic chemistry, and treats the subject in about the usual manner. Perhaps the character of it can be best expressed by a quotation from the preface. After advising the student to "go through the difficulties and not over or around them"—advice which some students will take and some will not—the author says that he has "endeavored to present the subject in a sufficiently elementary manner so as not to be beyond the grasp of the student in his first course in organic chemistry, yet to make the book comprehesive in that it covers the entire field by taking up practically all the important groups of compounds." An examination of the work indicates that this intention has in the main been reached. The book contains a very large amount of information well expressed and well arranged. It may be a question whether the structural formulas, which are very numerous, are not printed with type unnecessarily large. This takes up space and makes the book much larger for the proportion of the matter in it. One criticism must be expressed. The benzene ring-symbol is almost always wrongly written. Kekulé, in his original paper (*Ann. d. Chem. u. Pharm.*, 1865, v. 137, 129) suggested that the plain hexagon should be taken to indicate the unmodified benzene, and gives a drawing consisting of such a figure with a suggested orientation by letters. To attach CH groups to the angles of such a figure, as is done frequently in this work, is to indicate compounds much more complicated than really intended, and to burden the student with unnecessary symbols. The hexagons used have the same defect as noted above in the other formulas, for they are much too large.

Only a brief notice is given to enzymic action, and the term "catalysis," though noted in the book, is not in the index. Methyl alcohol is stated to have a disagreeable odor and to be used in denaturing common alcohol, but these statements concern the crude product, although given in connection with the description of the pure substance.

HENRY LEFFMANN.

ESSENTIALS OF PHYSICS. By George A. Hoadley, C.E., Sc.D., Swarthmore College. Revised editon, 534 pages, index and 556 illustrations, including several full page plates, 12mo. New York City, N. Y., American Book Company, 1921.

PHILOSOPHY AND THE NEW PHYSICS. An Essay on the Relativity Theory and the Theory of Quanta. By Louis Rougier, Docteur ès Lettres. Authorized translation from the author's corrected text by Morton Masius, M.A., Ph.D., Worcester Polytechnic Institute. xv–155 pages and index, 12mo. Philadelphia, P. Blakiston's Son & Co. $1.75 net.

Placed thus, these two books may be said to represent the alpha and omega of modern physics. Dr. Hoadley's book deals in a simple and straightforward manner with the whole field of physics as proper to be presented to the beginner; Rougier deals with abtruse phases of it, one portion of which—relativity—has obtained a fame that has made the term almost a household word.

Taking the "Essentials" first—as is, of course, the natural method—we find that in small and convenient compass, the author has embodied the general principles of physical science as known to-day, and the many applications of them to affairs of practical life and to further research. One of the interesting additions is the notice of camouflage. The allusion to the value of protective resemblance in animals, shows how the science of inanimate things spreads into biology, while the vivid pictures of the camouflaged liner given as a frontispiece, renews our thoughts of the expedients of the late war. The extensive use of illustrations serves greatly to aid the student and to awaken his or her interest. The convenient size and clear type will be of no little service in its use as a text-book.

Turning to the other work, we find first, a brief preface in which the translator suggests that the book seems to mark a measurable advance towards a junction of the broad streams of philosophy and scientific inquiry. It is within the memory of many teachers, now living, that physics was generally called natural philosophy, a phrase which apparently lingered longer among English-speaking scientists than with those of other nations, for Taine says in one of his essays, somewhat deprecatingly, "In England, a barometer is still called a philosophic instrument." Physicists, Dr. Masius tells us, are usually too much occupied with their special fields to give attention to philosophic inquiry; it is probably true of most of them that the mental make-up leads them to be impatient of the methods of philosophers. Yet the whole voice of science is suggestive of monism, that is, a unity of origin and an identity of type, and it is opportune, therefore, in this very decidedly materialistic age that those who can deal with the fields of physics and metaphysics, as they may be conveniently grouped should take up the burden of joining them. The present work is an

effort in this direction by a competent writer, and his French text has been admirably converted into English, so that a wider field of usefulness for the work will be secured.

HENRY LEFFMANN.

ECONOMIC PAPERS ON KENTUCKY GEOLOGY. By Willard Rouse Jillson, Director and State Geologist. x–295 pages, index and 115 illustrations (maps, diagrams and photographs), 8vo. Frankfort, Ky., The Kentucky Geological Survey, 1921.

This is a collection of papers and reports on the geology of some of the important deposits in the State of Kentucky, oil and gas, shale, asphalt rock and fluor-spar being especially noted. The Kentucky-West Virginia region is evidently of great potential importance in view of the trend of industrial life, and the study of any portion of it by a competent authority is welcome. Mr. Jillson has given us a very interesting and valuable book. One of the most important of the papers is that on rock asphalt. The earnest movement towards better roads will find no more appropriate direction than in the southern States, and we learn from the book that the discovery of a deposit of rock asphalt in Kentucky means a material improvement in the roads of the State. Unlike the asphalts so much used in our eastern cities, which are artificial mixtures of soft material with various minerals, the Kentucky product, like that of France, is ready for use when mined, and is laid without heating. Analysis shows that the mineral base of the rock is a true siliceous sand, each grain being coated with the bituminous material. Analyses are given showing nearly 7 per cent. of asphalt in the rock as mined. In addition to the information about asphalt, interesting articles are given on oil shales, oil and gas, and notes on general surface geology. The book is a most timely contribution to economic geology, and the numerous photographs make it interesting even to the general reader.

HENRY LEFFMANN.

VOLUMETRIC ANALYSIS FOR STUDENTS OF PHARMACEUTICAL AND GENERAL CHEMISTRY. By Charles H. Hampshire, B.Sc., F.I.C. Third edition, 120 pages and index, 12mo. Philadelphia, P. Blakiston's Son and Company, 1921. $1.75 net.

Though a small book, this contains a large amount of information, as the type used is proportionate in size. It is, however, quite clear and easily read. The text is arranged for special lines of work, and is divided into two styles, the paragraphs in larger type being descriptions of standard methods generally applicable, while those in smaller type are especially intended to exercise the student in the procedures of the British Pharmacopeia. Metric weights and measures are used throughout, which is gratifying, but it is to be regretted that the spelling "gramme" is retained. The reactions involved in the processes are mostly explained by equations. The typography, paper and printing are exceptionally good, and the size of the book makes it very convenient for the student. It is probable that in many of the procedures the book will be adapted to American methods, but American teachers will have to keep always in mind the liability of British methods to depart from our practise.

HENRY LEFFMANN.

PUBLICATIONS RECEIVED.

Volumetric Analysis for Students of Pharmaceutical and General Chemistry, by Charles H. Hampshire, B.Sc. (London), F.I.C. Third edition, 124 pages, illustrations, 12mo. Philadelphia, P. Blakiston's Son & Company, 1921. Price $1.75 net.

Statique Dynamique, par M. Steyvaert, Correspondant de l'Académie Royale de Belgique, Professeur à l'Université de Gand. 205 pages, illustrations, 8vo. Gand, Van Rysselberghe & Rombaut, 1920. Price, in paper, 20 francs.

National Advisory Committee for Aeronautics: Technical Notes No. 52, A New Method of Testing Models in Wind Tunnels, by W. Margoulis. 19 pages, plate, quarto. No. 57, The Caproni Seaplane, by Max Munk. 14 pages, photograph, quarto. No. 59, The Dynamometer Hub for Testing Propellers and Engines During Flight, by O. Enoch. 18 pages, plates, quarto. Washington, Committee 1921. Report No. 111, The Variation of Aerofoil Lift and Drag Coefficients with Changes in Size and Speed, by Walter S. Diehl. 10 pages, diagrams, quarto. Washington, Government Printing Office, 1921.

Some Experiments on Thermal Diffusion. T. L. IBBS. (*Proc. Royal Society*, A 700.)—From theoretical considerations Dr. S. Chapman deduced that "a temperature gradient applied to a uniform mixture of two gases will tend to produce non-uniformity of composition, the heavier and larger molecules diffusing toward the cooler side, and the smaller and lighter molecules diffusing toward the hotter side. This phenomenon was termed 'thermal diffusion.' The difference in composition due to thermal diffusion increases until it is balanced by the opposite effects of ordinary diffusion, when a steady state will be reached. The effect is greatest when the gases are mixed in nearly equal proportions by volume, and will be greater the more unequal are the masses and diameters of the gas molecules."

A mixture of hydrogen and carbon dioxide was used. This flowed through a cylinder in the axis of which was a spiral of platinum wire heated by an electrical current. The gas from the peripheral part of the cylinder and that from the axial part escaped by different outlets. The two streams of gas were led into a differential katharometer calibrated so that the deflections of its galvanometer could be translated into composition differences. The excess of hydrogen was always found on the side of the hotter gas. Raising the temperature of the spiral increased the degree of separation. For instance, in the case of one mixture raising its temperature from 25 to 570° C. caused the percentage separation to increase from .045 to 3.52. In all cases maximum separation was got for mixtures containing from 50 to 60 per cent. of hydrogen. Only about three minutes was needed for this thermal diffusion to take place.

G. F. S.

Preparation of Plates for Ultra-violet Light.—Photography offers the only satisfactory method for recording the phenomena of ultra-violet and infra-red light, inasmuch as these rays are invisible to the unaided eye. It is fortunate, indeed, that the ordinary silver salts are sensitive over so wide a range. It is true that many of the invisible rays can be observed by means of a fluorescent screen, but this does not furnish a permanent record. Research has shown a wide range of both forms of rays, and one serious interference with investigations is that ordinary glass is practically opaque to all vibrations above the violet, so that resort must be made to quartz, fluospar and other substances, either rare or difficult to obtain in clear condition. The recent inventions, however, by which quartz can be cast in the form of clear plates and tubes will be of advantage in these researches. In a recent issue of the *Journal de Physique* (1921, ii, 156) Duclaux and Jeantet describe a method of treating ordinary plates so as to increase greatly the sensitiveness to the higher vibrations. They had need of plates sensitive beyond 1900 angström, and tried the procedure advocated by Schumann, but found it tedious and uncertain. Schumann plates are distinguished by the small proportion of gelatin, and it was thought that this condition could be secured by degelatinizing to a great extent ordinary plates. Trials of various methods, such as immersion in warm water, acid solutions, digestive enzyms, were without success, but a simple and satisfactory procedure was devised.

The plate is placed horizontally in a dish with dilute sulphuric acid (one volume of the strong acid to ten volumes of water), and kept for four hours at room temperature (about 77° F.), the temperature being a little higher than this at the beginning and a little lower at the end. They are then removed to a dish in which they are washed by a very slow current of water, as the remaining gelatin is tender. Thirty minutes will be a sufficient washing. They are then dried, which requires but little time on account of the small amount of gelatin present. Plates thus treated retain a thin layer of emulsion poor in gelatin and uniformly spread on the glass. This deposit is extremely sensitive to ultra-violet rays, but is also very fragile, and the authors recommend that before developing the surface should be coated with a thin film of collodion, the plate being immersed in the developing bath before collodion is quite dry. Although most commercial plates are adapted fairly well for this procedure, it is likely that trial with many forms will show some more suitable than others. For rays of much greater wave-length than above noted, these plates are ten times more sensitive than the best plates prepared according to Schumann's method, and at least 200 times as sensitive as the plate in its commercial form.

Another method for obtaining plates of high sensitiveness to short wave-lengths is by covering the emulsion with a layer of fluorescent substance. Such a substance absorbs, so to speak, the short waves and emits in turn waves of greater length, to which the gelatin is

transparent, and thus permits an action on the silver compound, hence the impression is made as if the gelatin was not present. For this method, substances giving blue or violet fluorescence should be chosen, and they should be dissolved in a liquid that will not swell the gelatin, and is not absorbed by it, since the efficiency of the process depends on the fact that the fluorescent rays act before the light enters the gelatin film. Water is, therefore, not applicable. The authors obtained good results with a solution of esculin in glycerol, but found most satisfactory results with lubricating oil. Many of the commercial forms of these have a distinct fluorescence due to hydrocarbons. It is sufficient to smear a few drops of such an oil over the emulsion by means of a wad of cotton. After exposure this film should be removed by means of ether or alcohol. A very thin fluorescent layer may be obtained by immersing the plate for a few minutes in a solution of the fluorescent oil in light petroleum or alcohol and allowing the solvent to evaporate. These procedures are simple and effective. They enable the operator to secure photographs of rays ranging from the extreme red to the limit of the ultra-violet. One slight defect is noted, a very small enlargement of the rays by irradiation, but this does not go beyond the twentieth of a millimetre.

The processes have been tried with many commercial plates, and the sensibility is found to be greater than with the sulphuric acid method. It is possible, indeed, to carry out an instantaneous spectrography. Detailed results with certain metallic spectra are given in the paper. H. L.

CURRENT TOPICS.

Studies of Desensitizers.—The interest in the application of phenosafranin in desensitizing photographic emulsions has led to many investigations, among which the most extensive so far reported are those communicated by A. and L. Lumière and Seyewetz to a recent meeting of the French Society of Photography and published in full in the current number of the society's journal (*Bull. Soc. Fran. d. Photog.*, **3**, 1921, viii, 144). An abstract giving the principal data of the paper is herewith presented, taken from the *Photographic Journal of America*.

The inventor of the process, Lüppo-Cramer, has presented a large amount of information concerning it in special work. One property of phenosafranin is some objection to its use, namely, its high staining power on gelatin, and if a substance of equal power to desensitize but which has no staining power could be secured, a notable gain would be made.

Phenosafranin belongs to a type of colors containing what is termed a phenazin nucleus, and Lumière and Seyewetz directed the investigation first to ascertain if this particular molecular structure is the only one that determines the peculiar property or if other classes of bodies possess it. Further, it was advisable to ascertain if the desensitizing action is extended to panchromatic plates. The interesting question arises whether, if several desensitizers of apparently equal power are available, each has a specific adaptability to a given case. Lumière and Seyewetz also raise the question as to whether the action is chemical, physical or chemico-physical. It seems hardly worth while to discuss such a question until we have clearly determined what is meant by the several terms.

Fourteen colors, all forms of the safranin type, were tried in comparison with safranin. Of these, toluosafranin is doubtless nearest in composition to the standard. None of these substances was found to be fully equal to phenosafranin, and many were decidedly inferior. One of them, a very complicated derivative, was found to destroy to a certain extent the latent image, another was but slightly soluble, and one that approached closely the power of the standard, colors the gelatin quite obstinately. Several of them are not commercial articles, but were specially prepared by the investigators.

In the experiments, plates of the highest sensitiveness (not panchromatic) were used, all receiving the same exposure, and afterward immersed for two minutes in solutions of the colors ranging from 1 to 100 to 1 to 2000 in strength. The stronger solutions were first used, and then the tests made in solutions of gradually decreas-

ing strength. Development was carried out by the light of a candle 1.5 metres distant (about 5 feet), the light being reflected directly upon the developing dish, so as to have the plate exposed from above. The developer was paraminophenol, allowed to act for four minutes at about 17° C. (63° F.), the image being examined by transmitted light twice during the procedure, first after two minutes and then after three and a half minutes. If the picture thus obtained showed only a slight veiling, the procedure was repeated with panchromatic plates. The experiments seemed to indicate that certain molecular conditions are necessary to the desensitizing action—the presence of a phenazin nucleus and the substitution of amidogen groups in the benzene ring—but, notwithstanding that these conditions are fulfilled in the color known as neutral violet it has no marked desensitizing powers, and is inferior to neutral red, which approximates to phenosafranin in its action. Neutral red is a brownish-red dye, having little brilliancy, coloring gelatin only faintly, and much more easily removed than the safranins, but the proper impregnation of the plate to secure full measure of desensitization requires double the time that is required by the standard, that is, four minutes instead of two minutes As noted above, the colors tried were not found to possess quite the power of phenosafranin, but one of them, chresosafranin, can be removed very quickly from the film.

The tests were then extended to panchromatic plates of wide range of color sensitiveness. The procedure was to photograph spectra on each plate, immerse, in darkness, for one minute, develop for one and a half minutes, also in darkness with diaminophenol, then continue development for two and a half minutes at 20 inches from a 16 c.p. electric bulb, covered by yellow papers, stained with tartrazin, giving a very good illumination. During this latter period the plates were examined by transmitted light four times for a period of three seconds each. Most of the substances tried were unsatisfactory, but tolyluene red and aurantia (a yellow dye) gave distinct desensitizing action.

The paper contains also the results of trials of substances not related to the safranins. Among these a drug, apomorphin hydrochlorid, was found to have a distinct effect, a somewhat remarkable fact. Picric acid is also not without effect, but acts more like a screen and the action differs with the strength of the solution.

They conclude that the action of the safranins is a specific desensitization and not a mere screening, inasmuch as the violet dyes of the type act as well as the red; moreover, if, before development, the color is washed out, the sensitiveness slowly returns and when all color is gone, the plate has resumed its susceptibility to light. Phenosafranin is so far the only material eminently adapted to desensitization in general, but where complete immunity from red light is not indispensable, aurantia will be found applicable, as it does not stain the fingers, is more easily removed by washing, and does not

involve, as phenosafranin does, the occasional employment of a decolorizing solution.

The Fuel Problem.—No greater problem is before the industrial world to-day than the economical use of fuel. Among the most recent contributions to the subject is an address by Sir George T. Beilby, delivered in the theatre of the (British) Institution of Civil Engineers, and published in *Chemical News* (1921, v. 123, 86). The following figures taken from the report of the U. S. Geological Survey give the coal production, including lignite and brown coal, for 1920. The total output was 1300 million metric tons, of which

	Per cent.
The United States of America produced	45
Great Britain and the British Empire	22
Germany	19
Other countries, ranging from 2½ per cent. downward	14
	100

One of the most significant features revealed by this survey is the remarkably rapid development in the winning and use of brown coal and lignite in Europe and particularly Germany. The output in Germany in 1919 had reached the huge figure of 93.8 million tons; but this was overtopped in 1920 by an output of 111.6 million tons—an increase in one year of nearly 18 million tons. The total European output in 1920 amounted to 140.7 million tons, made up as follows:

	Million Tons.
Germany	111.6
Czecho-Slovakia	19.7
Jugo-Slovakia	2.5
Austria	2.5
Italy	1.6
Netherlands	1.4
France	1.0
Spain	0.5

The output of ordinary coal in Germany for 1920 was 140.8 million tons. The brown coal industry in that country is of old standing; and its rapid development in recent years is based on sound knowledge and experience. Though in its natural state a less concentrated fuel than bituminous or anthracite coal, brown coal has many points in its favor. The chief of these is the low cost at which it can be won as compared with ordinary coal. Where extensive deposits of great thickness occur, these can be worked opencast and excavated by machinery. The winning of brown coal is thus on

an altogether different basis from coal mining, with its deep and costly underground roads and workings, which involve heavy costs for timbering, pumping, and ventilation. The manual labor required is much smaller in amount for a given output, and is of less highly specialized type; while the special dangers and uncertainties of coal mining are practically absent. The capital charges, being mainly on surface roads and on excavating machinery, are relatively light, as compared with the heavy initial and permanent charges involved in the sinking and equipment of shafts or mines. Brown coal, though it contains from 40 to 60 per cent. of water, is to-day by far the cheapest source of thermal units. Its further manufacture by drying, briquetting, and carbonization can be carried out close to the point of excavation, and under conditions favorable to production on a large scale, and, therefore, at a low cost.

The glowing accounts of this development which have appeared in the technical press during the past two years may have struck us as exaggerated; but the solid fact remains that the output of lignite in Germany last year was 111 million tons. Germany has already faced the fuel problem of the future so far as she herself is concerned

H. L.

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